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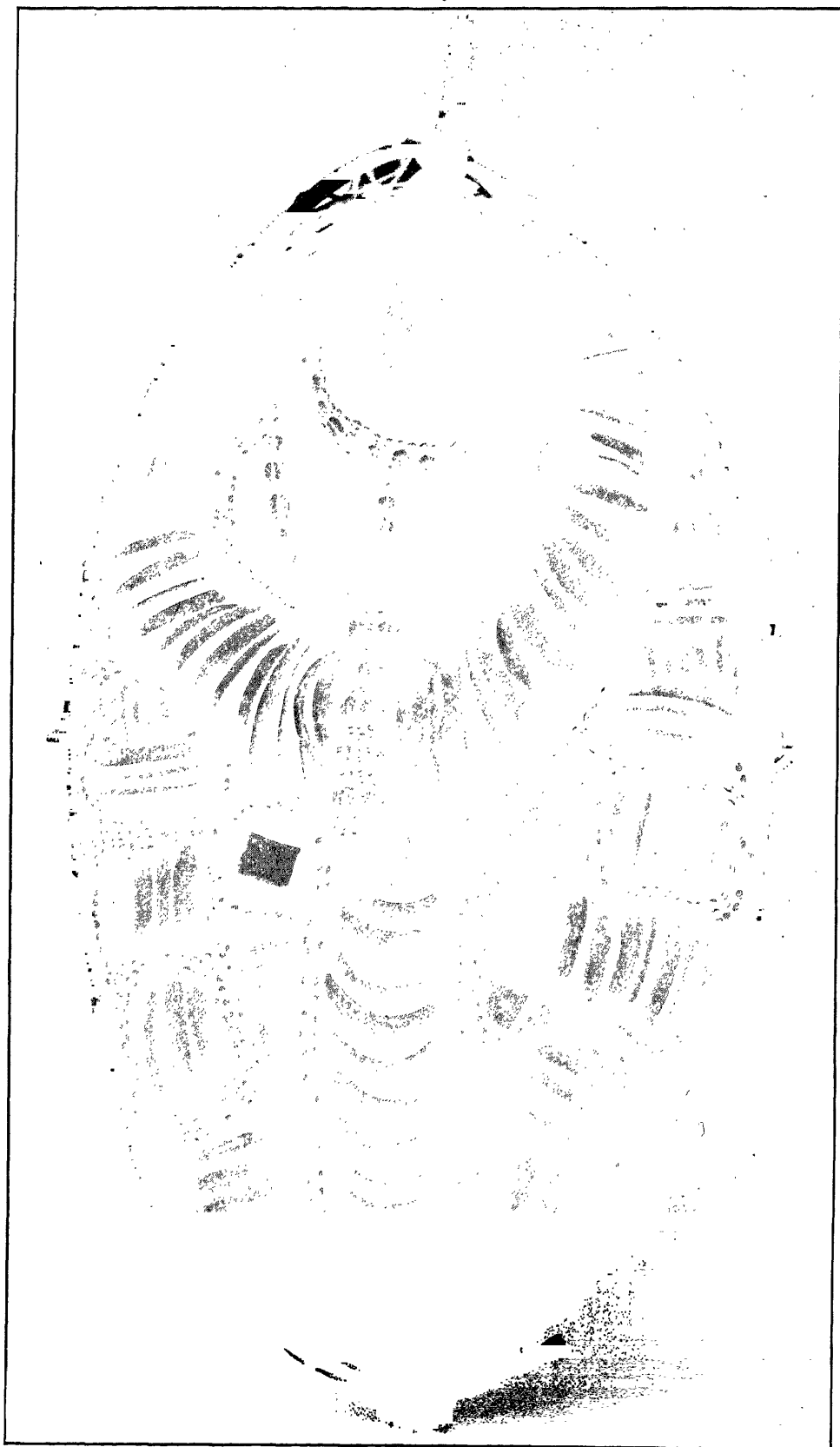
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A CEREMONIAL "MASK" FROM THE SEPIK RIVER, NEW GUINEA.

MAN

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N.B.—Articles published in MAN should be quoted by the year and the reference-number of the article, not by the page-reference: e.g., the article which begins on page 2 should be quoted as MAN, 1926, 2.

ORIGINAL ARTICLES.

With Plate A.

New Guinea: Technology.

Joyce.

A Ceremonial "Mask" from the Sepik River, New Guinea. *With*
Plate A. By T. A. Joyce.

Plate A. shows a ceremonial object from the Sepik River, district of New Guinea, which is of an unusual type, and is perhaps worthy of record. It is one of a series of objects presented to the British Museum by Mr. Schreiber, of Dutch New Guinea, which were collected by a recent explorer on the Sepik River, since dead. The collection includes a number of wooden shields, covered with feather-work mosaic, which were also ceremonial objects, since they were kept wrapped in sago leaves in the darkest portion of the ceremonial hut. I have termed this object a "mask," for reasons which will appear later, but, in truth, nothing is known of its use, and it shows no attachments suitable for a human head.

Morphologically it resembles a shield, long-oval in shape, composed of basket-work, coiled technique, supported by a stout rim of split bamboo, and, further, by a wooden stick fastened to the reverse surface in a longitudinal direction, which forms a kind of "backbone."

The face has been plastered with clay, in which the ornamentation has been inset. At the top is a human skull, secured by cane lashings (the illustration shows it practically *in norma verticalis*), ornamented with cowrie-shells, and furnished with a long artificial "nose," similarly decorated. Below the skull are semi-circular rows of shells and pig tusks, simulating a necklace (it is interesting to note that the handle of a china mug finds a place in the series of pig tusks). The rest of the field is decorated with other series of pig tusks, coix seeds, crocodile teeth, a couple of fragments of shell (? river mussel), and two broken bits of china. The culture of the tribes of the Sepik River is little known, and specimens from this region are extremely rare in English museums. A specimen, somewhat similar to that figured in Plate A, was published in *Ergebnisse der Südsee-Expedition 1908-10*, edited by Professor G. Thilenius, in the Vol. II. "Ethnographie, A. Melanesien, Band I," p. 401, by Otto Reiche (published under the auspices of the Hamburgische Wissenschaftliche Stiftung in 1913). The specimen there figured, which was collected at Kambrinum, about 100 miles from the mouth of the Sepik,

is smaller than the British Museum specimen, but shows the same technique, although only the front portion of the skull, together with the lower jaw, is there used. The author calls it a *Schädelmaske*, supposing it to be the earliest form of mask employed, and that is why I have used the term "mask" in my title. But he gives no details as to its use.

One of the reasons why I have ventured to submit this short note to MAN is the hope that some reader may be able to supply information as to the real significance of the object. Unfortunately the photo bears no scale, but the size of the specimen can be estimated from the skull, which is quite normal. The actual dimensions of the "mask" are 44 by 23 inches.

T. A. JOYCE.

Craniology.

Tildesley.

On the Application of Mathematical Formulæ for Estimating Skull-Capacity. By Miss M. L. Tildesley.

2

In his article on "The Chancelade Skull" in MAN, 98, 1925, Professor Sollas devotes some space* to the criticism (1) of Sir Arthur Keith's choice of a formula to apply to the Piltdown and Chancelade skulls from among those supplied by Lee and Pearson for the estimation of skull-capacity from linear measurements†; (2) of the value of the results obtained by applying even the formula which he himself selects from among them.

(1) As regards choice of formula: one must refer again to Lee and Pearson's paper. These authors examined various types of formulæ based on linear regression and selected as the most satisfactory an equation of the type: $\text{capacity} = \text{constant} \times (\text{length} \times \text{breadth} \times \text{auricular height}) + \text{constant}$. They then worked out the numerical values of these constants for males and females of three very diverse races, viz. Bavarian, Aino and Ancient Egyptian, and gave further *mean formulæ*‡ (a male and a female) with constants obtained by averaging those for the three races in question. As regards the application of these various formulæ, they state§ that the general rule for deducing the best result would clearly be to work with the formula for the most closely associated race, but if no association can be predicted, then with the mean formula.

Accordingly in estimating the Piltdown capacity, Sir Arthur Keith applied the mean formula (♀):

$$\text{capacity} = 0.0004 \times L \times B \times H + 206.$$

Prof. Sollas says he is unable to find this formula and suggests it is "a hybrid produced by crossing" the formulæ "for a man's skull and a woman's head." May one suggest that he look again on p. 243 of Lee and Pearson? Similarly that applied by Sir A. Keith to the Chancelade skull is the mean formula (♂). Prof. Sollas, on the other hand, selects the one for German males; as he himself suggests no German affinities he will admit that Sir Arthur Keith's selection was the more correct. However, although, in the case of a race not closely allied to one of the three given, the mean formulæ are the ones recommended, Lee and Pearson found that the application to *individual skulls* of the formula for another race did not give results very different from that for their own race, even though the races in question were as diverse as Germans, Ainos and Egyptians. They found that the average error in calculating the capacity of the individual skull from its own racial formula lay between 3 and 4 per cent.||, while the average error was still 3 to 4 per

* October, 1925, pp. 158-9.

† "Data for the Problem of Evolution in Man," by Lee and Pearson. *Phil. Trans.*, Vol. CXCVI, A.

‡ *Phil. Trans.*, loc. cit., p. 243.

§ *Phil. Trans.*, loc. cit., p. 245

|| *Phil. Trans.*, loc. cit., p. 238.

cent. if the formula for another race were used.* Later, Dr. Isserlis† worked out the formula-constants for the Negro skull (total height being used instead of auricular height) and showed that the application of this Negro-formula to German mean length, breadth and height gave a result very close to the German mean cranial capacity as found by direct measurement.

(2) To turn now to the accuracy of the results obtained by applying a formula : all that a formula claims to do for the individual skull is to give the *average capacity* of a number of skulls having its own length, breadth and auricular height, and to give a measure of the amount of variability among them. Their variability is indicated by the "probable error," a mathematical term which appears to be imperfectly understood in the paper before us. In applying the formula for German ♂s to the Chancelade skull, Prof. Sollas gets (or should get‡) for the capacity

$$0.000332 (193 \times 139 \times 124) + 415.34 \pm 55.41 = 1519.75 \pm 55.41.$$

Now the "probable error" ± 55.41 (a term which he changes to "possible error") does not indicate the limits within which the true skull-capacity may be expected to differ from the estimated value 1519.75 c.c.; it merely marks the boundaries within which, if a large number of skulls of the same length, breadth and height were measured, *half* of the results would fall, while the other half would differ by more than 55.41 from 1519.75. Twice the probable error (*i.e.* ± 110.82) gives the limits within which 10 out of every 11 should fall, but the 11th may be expected to fall outside. Two and a half times the probable error should include 21 out of 22 cases, and 3 times the probable error 45 out of 46. There is, therefore, no cause for dissatisfaction if in a long series of crania a skull can be found that differs from its estimated capacity by even more than 3 times the probable-error, though this, of course, should be rare.

Turning now to the Chancelade skull, is the difference between its true capacity and its estimated capacity such as to provide an argument for excluding it from the order of skulls to which the Lee-Pearson mean formula is applicable? Testut, who measured it, gives its capacity as about 1710 c.c., with a possible error of 5 to 10 c.c. He followed Broca's method of determining capacity,§ and of this method Lee and Pearson assert positively|| that it gives exaggerated results to an extent that they estimate at about 60 to 80 c.c. beyond the true value.

Taking only the smaller of these two figures we may anticipate that re-measurement of the capacity by more accurate methods would very probably reduce Testut's 1710 c.c. to round about 1650 c.c. Now the capacity as estimated

* *Phil. Trans.*, loc. cit., p. 242.

† *Biometrika*, Vol. 10, 1914-15, pp. 188-193. In this paper Dr. Isserlis does not deal with the average error when the Negro formula is applied to the individual German skull. But in using it to find the mean capacity of the German series (100 ♂ 99 ♀) he gets a result within 10 c.c. of the true mean capacity for the males, and 7 c.c. for the females : a result well within the average error of 1 per cent. which Lee and Pearson found in a similar use of a formula obtained from one of their races to determine the mean skull capacity of another (*Phil. Trans.*, loc. cit., p. 245).

‡ He makes two mistakes here, misquoting the second constant as 415.32 instead of 415.34, and working out the result to 1522.32 instead of 1519.75. It may be noted that in the course of this discussion he makes several other slips : he works out his application of the formula to Eskimo skull No. 146 as 1552.34 instead of 1551.68, and that to No. 237 as 1531.34 instead of 1531.20. Again, he refers the reader to "formula (9)" on p. 252 of Lee and Pearson's paper, though the only formula on this page is formula (14); he also supposes "formula (9)" by which Lee and Pearson designate a type of formula [*viz.*, $\text{cap.} = \text{constant} \times (L \times B \times H) + \text{constant}$] to indicate merely a particular example of this formula with constants for German ♂s inserted.

§ *Rev. de la Soc. d'Anthrop. de Lyon*, Vol. 8, 1889, pp. 158-160. The skull was in this case too fragile for shot to be employed, but he took pains to ensure getting the same result as by Broca's method.

|| *Phil. Trans.*, loc. cit., p. 248.

by the mean δ formula is 1537 c.c.* The difference between the two would, therefore, be only about twice the probable error, making the skull fall well within the usual range of variation in races to which the mean formula applies. If another skull of the Chancelade type should be found, too incomplete for direct measurement, then those who maintain (on other grounds) that Chancelade men have Eskimo affinities will make use of the Eskimo formula which is now worked out for us by Mr. L. H. Dudley Buxton†; while for those who agree with Sir Arthur Keith that they are "of a racial stock of true European kind"‡ the mean formula will be as appropriate as any at present available. It is to be hoped, however, that Prof. Karl Pearson and his collaborators may at some time carry further the investigations into capacity-formulæ which we owe to them already. In a study by Lewenz and Pearson of the relation between inter-racial *arc* measurements and cranial capacity§ it was found that the largest and smallest skulls in a series were those that provided the worst fit to the regression line equation, and it was suggested that a parabola might provide a better fit to a series than a straight line. This may perhaps be true also of the relation between diameters and capacity in skulls, and I venture very modestly to suggest that the large cranial capacity of so many of the Eskimo skulls may be the reason why the linear regression formulæ that apply well to races so diverse as German, Aino, Egyptian and Negro, are as unreliable as Mr. Buxton states when applied to Eskimo crania.

M. L. TILDESLEY.

Prehistory: Ice Age.

Peake: Fleure.

The Ice Age. By Harold J. E. Peake and Professor H. J. Fleure, D.Sc.

3

For many years it has been taken for granted that during the Pleistocene epoch the land in Europe stood at a higher level than at present as compared with the sea. This view was first advanced in 1880 by Sir William Boyd Dawkins|| to account for the spread of Pleistocene mammals into the British Isles. He fixed the ancient coast line provisionally at or near the 100 fathom line, because that was convenient for his purpose; it provided clearly for the conversion of the British Isles region as a whole into an extension of the continent. The 50 fathom line would have given connections from the continent through Britain to Ireland, but they would have been fragmentary.

This level has been accepted, as a matter of course, by later writers, and has been defended by the statement that this is the level of the top of a submerged cliff. The latter statement is not strictly true, except for a small zone off the north-west of Ireland, and, to some extent, a zone to the north of the Hebrides, where the 100 fathom line and the 1500 fathom line, at any rate, are fairly near one another. The top of the submerged cliff-line, throughout most of its length, is at a considerably lower level. The 100 fathom line has probably been chosen and retained for the convenient reason that it is one of the few bathymetrical contour lines shown on almost every map of Europe.

When, after the discoveries at Dürnten and Hötting, and the classic work of Penck and Brückner, the multiglacial view became prevalent, at any rate outside this country, it seems to have been assumed that intermittent stages of elevation and depression accompanied the intermittent periods of geniality and cold, and Dr. Fairfield Osborn¶ assumed that elevation synchronised with geniality, apparently because only during these periods could the *fauna* and *flora* have spread over the

* Taken correct to nearest integer.

† MAN, October, 1925, 97.

‡ *Antiquity of Man*, 1925, Vol. I, p. 86.

§ *Biometrika*, Vol. 3, 1904, p. 388.

|| W. Boyd Dawkins: "Early Man in Britain" (1880).

¶ H. Fairfield Osborn: "Men of the Old Stone Age" (*passim*).

now sunken channels. This argument is not, however, conclusive, for many of the animals could have moved as easily in the cold as in the genial periods, while, were the converse hypothesis true, the existing channels would have appeared only during the middle of the genial periods; moreover some animals failed to reach Ireland. We propose, therefore, to discuss the possibility of the view that on the one hand elevation and glaciation, and, on the other, depression and geniality, were synchronous.

Long ago Sir Charles Lyell insisted on geographical changes as explanations of climatic revolutions, and Alfred Russell Wallace tried to estimate the effects of closing the channels between Europe and Greenland. In his paper, written some twenty years ago but published only recently, thanks to Mr. C. E. P. Brooks, the late Dr. F. W. Harmer* worked up the idea of the climatic severity that would result from the above mentioned closing of the channels to the northwards. The general idea is that the sheer rise of land would give Scandinavia an important area of perpetual snow, that the cutting off of the Norway Sea from the North Atlantic would make the former very cold and that the bitter climate of Scandinavia would spread to Ireland and to Scotland. Both these regions would be much higher than nowadays and would have snow fields. Harmer thought that the diversion of the storm track, which would run from Davis Strait head-on to the coast of the English Channel, would be an even more powerful influence. In Dr. Harmer's paper it is supposed that the Davis Strait would not be converted into land and thus there would be a communication from the Arctic Seas north of America to the North Atlantic. More recently Dr. W. J. Humphreys has suggested that land elevation may be one, at any rate, of the causes of ice ages.†

Professor Sollas‡ has spread familiarity with the recent work of Depéret and Lamothe upon a series of raised beaches around the Mediterranean. It has been objected, in some quarters, that these beaches are not of so uniform a height as has been stated, but this is not surprising. The bed of the Mediterranean has been unstable and is still so, especially in its western basin; differential movements, subsequent to the formation of the beaches, are likely to have occurred. Thus, while we may feel some uncertainty as to the exact original elevation of the beaches, and so of the subsidence, it seems to us to be clear that the land around the Mediterranean has on four separate occasions been lower than at present as compared with the sea, for it seems, for the moment, unimportant whether this change was due to subsidence in the land or a rise in the ocean level.

Now, among the remains of molluscs left behind in the first three of these beaches are those of species now inhabiting somewhat warmer seas. The undoubted inference from this is that during periods of subsidence the climate was more genial than at present, and we must, therefore, equate the elevated with the glacial phases.

Further evidence bearing on this question comes from nearer home. Burkitt§ tells us that Marr has recently pointed out that when deposits containing Chellean implements were being laid down the land in East Anglia was subsiding, while during Mousterian times it was rising. Now the developed Chellean implements are believed by most students to date from the genial period of the Riss-Würm interglacial phase, while an earlier type was in use during the corresponding period in the Mindel-Riss. If our view is correct, the land would have been sinking during the earlier part and rising during the latter part of each of these interglacial periods, and indications of

* F. W. Harmer: "Further Remarks on the Meteorological Conditions of the Pleistocene Epoch," *Q. J. Met. Soc.*, 1925, I, 247.

† W. J. Humphreys: "Some Factors of Climatic Control," *The Scientific Monthly*, xx, 454 (May, 1925).

‡ W. J. Sollas: "Man and the Ice Age," *Geol. Mag.*, ix, 703 (Jan. 1923), and "Ancient Hunters," 3rd edn., 1924.

§ Miles Burkitt: "Prehistory," 2nd ed. (1925), preface to new ed.

a lower level of the land in the early and middle Pleistocene Ages seem to be appearing. Jersey shows evidence of a beach (undated) 125 feet above present sea level and brick-clays occur at still higher levels. Sea formed caves occur on that island 60-100 feet above the present sea level. Guernsey also shows evidences of old coast lines formed when the land lay deeper in the water than now.

The evidence from the Chellean deposits of East Anglia is inconclusive, but not hostile to our view. In the case of the Mousterian deposits, however, it is different. All are agreed that in western Europe these implements date from the time when the climate was becoming progressively colder as the Würm glaciation advanced. The rise of the land in East Anglia during this period confirms our hypothesis. Burkitt tells us, too, that similar movements have been observed in the Somme valley, but so far not elsewhere, and there has been a tendency to look upon this as a local earth movement; but as it coincides so well with the evidence from the Mediterranean, we may more than suspect that these movements were reasonably coincident in time, however they may have varied in amount, in different regions of Europe and its neighbouring lands which felt them.

Geologists in Scandinavia and Finland are agreed* that at the close of the Pliocene, and so presumably at the beginning of the Pleistocene, Scandinavia was elevated at least 1,000 metres above its present level. We have emphasised the words "at least"; for this view is based upon the existence of a continental shelf off the north-west of Norway, and the cliff-line suggests that the old coast line was somewhat, perhaps considerably, below the line of a present depth of 1,000 metres.

The 1,000 metre line is rather below the 500 fathom line shown on many maps. The latter, if it were the coast line, would expose almost all the Wyville-Thomson ridge above the sea level; only a narrow channel would be left to connect the Arctic and Atlantic Oceans. The elevation of the land by 1,000 metres, or perhaps a little over, would close this narrow channel and all connection between the two oceans would be cut off.

The raising of the land by 1,000 metres would automatically bring down the snow line and the terminations of the glaciers by the same amount. Should this continue for some centuries the increased cold in the mountain regions would serve to bring these down still further by several hundred metres. We might, therefore, expect to find the Scandinavian snow line at least 1,200 metres, or possibly more, below its present level. We add "possibly more" for, if the sea outside Norway were cut off from the Atlantic by a complete Wyville-Thomson ridge, the cold may well have been intense. On the other hand the precipitation may have been less. It should be noticed that bitter cold in Scandinavia would doubtless make Central Europe very cold too.

The North Atlantic would be entirely cut off, both north-east and north-west, from Arctic waters, and it seems legitimate to suppose that something might be gleaned as to the climatic conditions from a survey of the present ones in the North Pacific. The south-westerly winds in the North Pacific blow north of latitudes 40°-50° in summer, but occupy the area north of latitude 40° more thoroughly in winter, and it is said that water is apt to drift northward past Vancouver (about latitude 50°) in winter, but southward in summer. In any case, the storm tracks and ocean drifts are much less marked in the North Pacific than in the North Atlantic of the present day, for in the latter case the open way northward between Scotland and Iceland and through the Norway Sea creates a famous low pressure area and storm track, which at present temperatures makes for mildness but, with lower general temperatures, would help accumulation of snow. In the case of the North Pacific of nowadays the area is flanked by a large region north of the Arctic Circle, with

* *Handb. der reg. Geologie*, iv. 3. Högbom. Fennoskandia 1913. "Die Tertiarzeit," 127-9.

an air pressure of over 30·0 inches of mercury during a good part of the winter, whereas the North Atlantic low pressure, say a pressure of 29·8 inches or less, spreads northwards almost to latitude 80° beyond the north of Norway.

The view which we would suggest is that with the Labrador-Greenland-Iceland-Scotland land bridge complete the low pressure area and storm tracks of the North Atlantic were less marked than now and that, as now on the Vancouver coast, so then on the southern part of the British coasts, conditions favoured a dry rather sunny summer, Vancouver in most parts gets a dry July and August and more hours of sunshine than any part of Britain.

While, therefore, we think that Scandinavia and the Norway Sea must have been bitterly cold and the Scottish Highlands may have been glaciated, we do not think the glaciation need have extended to the English plain if there was a sunny dry summer in this latitude. We also doubt whether this glaciation was of very long duration.

Now it is well known that Penck and Brückner have found in the Alps abundant evidence of the first or Günz glaciation, when, according to Obermaier's* calculations the snow and glaciers descended about 1,200 metres below their present level; but no such evidence can be found in western France by Boule†, nor is it present in this country, where signs of three glaciations only have been found by Boswell and Reid Moir.‡ A corresponding cold period is, however, known represented by the Weybourn Crag and the Chillesford beds; these were laid down, we would suggest, either immediately before or after this elevation, unless, indeed, by its isolation, the Arctic then rose considerably above the general ocean level.

The second or Mindel glaciation seems to have been felt completely in the west. We judge that the elevation of the land was probably less than in the Günz and that there were passages by which water and ice could get through the previously existing land barrier that had stretched from Labrador to Europe. This means that the elevation was somewhat less than 500 fathoms. This would automatically bring down the snow line at once by 900 metres, but, since according to Obermaier it fell about 1,300 metres, the duration of this glaciation must have been considerable. If our rule of diminishing elevation is true the rise during the Riss glaciation must have been about 250 fathoms or approximately 600 metres, while the coast during the Würm was, perhaps, near the 100 fathom line, signifying a rise of nearly 250 metres. These figures are purely provisional and will doubtless need adjustment for regional differences of rise and fall, when the comparative study of raised beaches in western and north-western Europe has further developed.

Marine communication between the North Atlantic on the one hand and the American Arctic and the Norway Sea on the other would probably increase the amount of ice in the North Atlantic so long as the northern lands remained much higher than at present. The present storm track from the Atlantic towards the North Cape would develop and would at first mean heavy snowfalls and a wetter, if milder, climate for Britain; so there might be less melting of snow than previously in the west. The smaller rises in the later glacial phases would reduce the extent of snow in the north and the West-European climate would be less severe than in the earlier glacial phases, especially after the present relations of land and sea became established in the West Indies. This last factor may have played a considerable part, as it has contributed a considerable amount of additional warmth to the waters of the south-west region of the North Atlantic, and so has helped to milden temperatures along the great low-pressure-and-storm track that now runs past the Hebrides to the north of Norway.

* H. Obermaier : " Fossil Man in Spain," p. 27.

† M. Boule : " Fossil Men " (1923), 36.

‡ P. G. H. Boswell and J. Reid Moir : " The Pleistocene Deposits, etc., at Foxhall Road, Ipswich," *J.R.A.I.*, liii, 229.

If this view be accepted, the actual figures will not be very different from those given in the following table :—

—	Raised Beaches.	Descent of Snow.	Subsidence.	Elevation.
	Depéret—Metres.	Obermaier—Metres.	Feet.	Feet.
Günz - - -	—	1,200	—	3,600
Günz-Mindel - - -	90-100	—	300	—
Mindel - - -	—	1,300	—	2,400
Mindel-Riss - - -	55-60	—	200	—
Riss - - -	—	1,300	—	1,600
Riss-Würm - - -	30-33	—	100	—
Würm - - -	—	1,200	—	800

The suggestion in this paper is that the Pleistocene period witnessed oscillations of the relations of land and sea in North-west Europe. That these oscillations were the echoes, as it were, of the great Alpine movements of the Oligocene and Miocene, and that along with these oscillations there may well have been eustatic movements, locking up water in periods of glacial growth and increasing the volume of the seas in periods of glacial decline. The estimates are necessarily provisional. The effects of elevation and depression upon climate in North-west Europe would be open to much misinterpretation were we to leave out of consideration the Wyville-Thomson ridge.

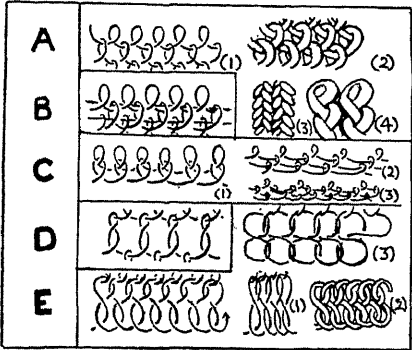
H. J. E. PEAKE,
H. J. FLEURE.

Technology.

Thomas.

Netting without a Knot. By Ernest S. Thomas.

In the accompanying figure, types of knotless netting are shown, which are *prima facie* developments of the simple looping (Examples of type A have



4
been quoted and described in MAN, 1909, 20; 1924, 113), showing that it is or was known to the Warundi of Ruanda, the Angoni, the Mangbettu, the Ituri pygmies (of L. Nyassa region?), the Indians of Brazil; in modern Peru, Fazoghli, and lastly, in ancient Egypt for basketry.

By the kindness of Mr. Henry Balfour, I am able to refer to the following examples of types B, C, D and E, which are all in the Pitt Rivers Museum, together, for completeness, with further examples of the simple technique A.

A. (1) Loosely worked.

- Dilly bags of cord. S. Australia, and N. S. Wales.
- Fibre net bag, Yungman Tribe, N. Territory, Australia.
- Bag used when diving for oysters, Aborigines, Tasmania.

Bag worked in bands of russet, cream, and black of fine string, long semi-oval in form, used by royalty. Uganda.

(2) Tightly worked.

Small semi-oval bag worked in stripes with very fine stiff brown and yellow thread. The bag is 9½ cm. long and has 12 lines to the cm. It was used for coffee beans and belonged to a former king. Uganda.

Woollen bag striped in five different colours. L. Eyre, S. Australia.

A round mat, sewn up tightly as a packing for pituri, of very fluffy woollen cord. Innaminka, Cooper's Creek, S. Australia.

(3) This is a long quiver-shaped bag, used for tobacco, made of rafia fibre. Every turn is drawn so tight that the top of the loop is pulled out very thin and submerged. The resultant effect is that of close plaiting. The technique is shown diagrammatically in (4).

The bag is a striking object, with long strips of twisted otter-tail fur pendant from the mouth, and a broad fur sling. Batutsi Tribe, Ruanda, Belgian E. Africa.

A similar bag is made on the border of Lake Kivu, S.W. Uganda.

B. *Like A, with a supplementary horizontal cord.*

Dilly bag. N. S. Wales.*

A round flat bag 11 cm. long with a short neck (like a flask). A long-rayed star of a different coloured string decorates each face. The loops are tightly drawn. Mexico.

C. (1) *The same as A except that the loops in a lower line are about the necks of those in the line above.*

The technique can be imitated in knitting by turning over each stitch.

Bag with handle and tassels, patterned in a different coloured string. Tightly worked. New Guinea.

Bag of woolly cord patterned with white edged black lozenges in a black grille. New Guinea.

(2), (3) *Here a decorative effect is obtained by varying the position of the loops from line to line.*

(2) Leg band. Port Moresby, British New Guinea.

(3) A small basket or bag of flax fibre beautifully constructed, and ornamented by variations on this theme, as well as with knots, forming festoon patterns. Probably New Zealand.

D. *Here the loop is given an extra turn figure-of-eight wise.*

Large carrying-bag of coarse fibre cord. Eastern Sudan.

String bag. Richmond River, N. S. Wales.

D and A (1). A finely made bag of sinew cord, mainly of the A (1) technique in yellow, with bands of D in red cord. Great Slave Lake, Canada.

E. *The upper loops of the figure-of-eight type are interlinked.*

This is a very efficient and popular netting. The effect varies greatly according to the thickness and kind of cord used and the size of the mesh. The best examples are from New Guinea (notably from the Yada Valley and Gira River), and the Chaco Boreal (Lengua Indians), Paraguay, S. America. The latter favour type (2) in which the work is drawn very tight so that the effect is that of knitting.

Most of the examples of their work are sausage-shaped pouches with a longitudinal mouth and sling handle.

The Lengua introduce very intricate patterns into their work with different coloured string. The New Guinea peoples do so also in their looser technique (1), but usually prefer variations of the castellate line design.

Other examples of (1).

A small bag of fine string, striped blue, red, and white. Baining, Gazelle Pa, New Britain.

A bag of fine cord, New Caledonia.

A bag of thickish brown string, Santiago, Peru.

An oblong string garment with shoulder straps, all in one piece, with striped pattern. Mataco Indians, Pilcomayo, N. Argentine.

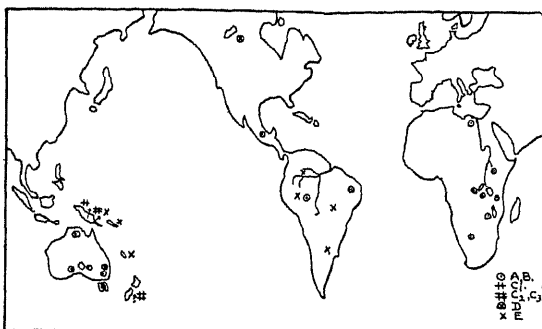
* An example of the Tasmanian Aborigines is given by Ling Roth.

E (1) and D (1) (alternate). A large bag with castellate pattern in brown string on white. Port Moresby, B. New Guinea.

E (3). A small bag of white stiff fibre cord (flax?) in which the effect is of square meshes. It is beautifully made, with lines of black cord at intervals. Ucayali R., Upper Amazon.

All the patterned bags are, without exception, worked with coloured strings, and are not stamped, stencilled, or painted.

Each of the above-described *motifs* can be readily imitated with needle and mesh, but the mesh is not needed for the tightly drawn simple looping of (A) types. The patterns are possibly produced by working with several needles simultaneously;



one, *i.e.*, for each colour. The background thread never seems to be carried behind straight across the lines of a pattern running vertically, but the spaces are filled in* with a minimum of discontinuity and irregularity in the work, necessitating, one would judge, a high degree of clear and intelligent thought.

The interest of this type of netting seems to have escaped the notice of ethnologists, as no description of the technique or instruments employed, or even illustrations of the finished articles, seem to have been published.

From its curious distribution, roughly shown on the accompanying sketch map, knotless netting is surely of considerable interest and importance ethnologically, and it is hoped that this note may serve to draw a measure of attention to this aspect of the subject.

ERNEST S. THOMAS.

Archæology.

Burkitt.

Archæological Notes. By M. C. Burkitt.

I am offering no apologies for writing notes that are disjointed and not interrelated. One of the most valuable and unique features of MAN is that it provides space where the odd fact or the chance idea can be spread abroad or discussed without having recourse to long, elaborately prepared articles.

The so-called Badarian-Solutrean question has been well to the fore of late, and Professor Petrie and others have given us their views in *The Times* and in MAN. As far as one can see, Professor Petrie rests his correlation of the two cultures on the following grounds: (1) As the date he assigns to the Badarian industries is 13,000 B.C., and as the date for the European Solutrean industries is ditto, then the two cultures must be the same. (2) The types of tools occurring at Badari and in the Solutrean industries are similar. The world owes a great deal to Professor Petrie, and, without his magnificent work, much of our knowledge on Egypt would be still lacking; but surely, on this small point, which is rather a question of Prehistory than of Egyptology, he is not perhaps quite correct? As regards his first premise, though not myself an Egyptologist, I believe it is true to say that Professor Petrie's chronology is not universally accepted, and so to build evidence on the assumption that the Badarian culture dates back to

* Perhaps by continually reversing the direction of netting, instead of going round spirally, when required. Many cases have been noticed where the patterning cord has been carefully spliced on to a string of the background colour: whether at the beginning or end of patterning, or either, would require a closer study of the intricate technique than the interest perhaps warrants.

13,000 B.C. is hardly safe. I fancy there are those who would differ from this estimate by as much as three or four thousand years. From the point of view of the dating of the Solutrean in Europe, while personally agreeing with Professor Petrie that 13,000 B.C. is a possible date, it cannot be claimed that this so-called shorter chronology is by any means universally accepted. The first premise, therefore, falls to the ground. As regards the second, I can only plead that, having seen most of the Badarian industries at those most interesting yearly expositions in London, I cannot admit any great similarity with Solutrean industries of Europe. I need not add that the presence at Badari of pottery with sometimes a kind of ripple ware that appears to be first cousin to the well-known Neolithic ripple ware of Crete, still further militates against this most interesting culture being really of Palæolithic age.

Further diggings were continued at the cave near Yealmpton in South Devon, described in my last Archaeological Notes in MAN. The results obtained this year were, perhaps, rather disappointing. The cave opens high up on the edge of what had been at first considered as an ancient quarry, but is now thought to have been a narrow dingle, at right angles to the River Yealm, opened out later as a quarry. The cave tunnels right through a narrow limestone spur, the other entrance lying above the main valley of the Yealm. Burrowing animals have completely disturbed any stratigraphy that might have existed, and so evidence from animal bones is unobtainable. The remains of these burrowing animals were frequent in all states of preservation, including a fossilised horse toe. There were also found human arm bones, the rest of the skeleton being absent. They were found in the superficial layers, however, and there is no evidence whatsoever that they are in any way prehistoric. About the middle of the deposit (some 5 feet thick) occurred an irregular hearth a few inches thick. On excavation, this did not prove to be of any very great size, ceasing half-way across the mouth of the cave altogether, and only penetrating a few feet inwards. This latter fact proves that we were dealing with the ancient entrance of the cave and not, as had been thought last year, with the interior exposed by quarrying operations. A few more flints were found just under and about the hearth; these consisted solely of flakes which had a deep *patina* and resembled closely similar flakes of Palæolithic age from the industries at Kent's Cavern on the one hand and Burrington Cave in Mendip on the other. The nearest natural flint deposits are miles away. No trace of pottery was found. Thus the slight evidence remains in favour of a small Palæolithic hunting station, and it is hoped that more positive proof will be found next year when the unexcavated portion of the floor is dug.

Having recently assisted at an interesting Neolithic dig in Burgenland, on the borders between Austria and Hungary, under the most competent direction of Dr. Adolf Mahr, of the Museum at Vienna, I should like briefly to describe the excellent methods employed. The site was on a sloping field below the road from Kaisersteinbruch to Sommerain, close to a very good spring. The whole field was first surveyed, so that the site of the dig could be absolutely accurately placed. A main trench was then dug parallel to a line *AB*, which was divided into metres and centimetres. Looking along the line *AB*, all objects found to the left were marked "L" those to the right "R." Every important piece of decorated band pottery or other object found could thus be instantly located as *x* metres along *AB*, *y* centimetres left or right, *z* centimetres below *AB*. The ease with which squared-paper plans could thus be made and an exact location for each object found was remarkable. The line *AB* was, of course, accurately placed in the general surveying of the field. Incidentally, the Neolithic level yielded a number of linear-keramic sherds, decorated with the well-known spiral meander, *noten-schrift*, and other motifs, flint and obsidian tools, etc.

Later, I had occasion to study briefly a part of the terraces of the Danube below Melck, and to discuss afresh the whole problem of glacial chronology. As is well known, Dr. Bayer has a chronological system of his own which has been severely handled by Dr. Obermaier, Dr. Schmidt of Tübingen, the French School generally, and others. On the other hand, I am bound to say that in some respects it agrees closely with the English evidence. It is out of place here to describe the system in detail; a *résumé* will be found in a new article by Dr. Bayer, "Die ältere Steinzeit in den Sudetenländern." Heft 2, 1925. It is sufficient to say here that two main glaciations are recognised, with a long, warm interglacial period between them. The later glacial period shows a slight recession or shrinking of the ice in the middle, and associated with this last glaciation and shrinking are a number of loess deposits. Roughly speaking, the older ice age of Dr. Bayer coincides with the Günz, Günz-Mindel and Mindel of the Penckian system, the long, warm interglacial period with the Mindel-Riss, while the early part of his second glacial coincides with the Riss, the recession of the ice to the Riss-Würm, and the second maxima to the Würm glaciation. Further, Dr. Bayer considers that the main portion of the Lower Palæolithic industries have to be assigned to this interglacial period, the Mousterian to the early portion of his second glaciation, the Aurignacian to the recession of the ice, the Solutrean to the second maximum, the Magdalenian as usual remaining just post-glacial. I only here recall Dr. Bayer's scheme for criticism by others, and I am by no means convinced that he is right. On the other hand, it is perfectly true that there are two, and only two, great "drifts" in East Anglia; for the cold climate of the Chillsford beds, followed by the slight warming at the base of the Cromer Forest series, would seem hardly sufficient to postulate a glaciation and an interglacial period, and the same might be said in respect to Sandford's discoveries in the Oxford district of a period of denudation (? Riss-Würm in age) lying between the top of the cold Wolvercote channel series and the cold gravels of the buried channel. Should Dr. Bayer be in part at least correct, we should have to consider the lower part of the chalky boulder clay as Riss, the upper part as Würm, and, there being no very marked Riss-Würm interglaciation, it would still be correct to say that our Mousterian is associated with the last great glacial manifestation in East Anglia, but wrong to class it as Würmian, rather correlating it with the Riss of Penck. However, I am by no means convinced and am very loath to disregard so much evidence that has accumulated to show that Neanderthal man is of Würm age.

Incidentally, my attention was drawn to the importance of considering the under-topography of a valley when studying the terraces. Should the under-topography show step formation, then the gravel terraces on these steps were, no doubt, formed in the orthodox way by the shrinking and swelling of the rivers in glacial and interglacial times. On the other hand, if the under-topography of the valley is U-shaped and shows no step formation, then the state of affairs may have been that, on the retreat of a glacier or from any other cause, the valley was completely filled with *débris*, which latter has been removed by successive deepening of the valley bed, leaving a series of terraces at various points along the sides. It should be noted that in the former case the lowest terrace is the newest, the upper terrace the oldest; in the second case, as the terraces are formed by being merely left by denudation, the mass of the gravel being due to aggradation, the top terrace is the newest, the bottom terrace the oldest.

A further visit to Moravia was undertaken, and testimony must be given to the importance of this country to the student of both Palæolithic and Neolithic cultures. It is not too much to say that Moravia is second only to France in the wealth of its Palæolithic industries, while it is almost impossible to understand the Neolithic succession in East Central Europe without studying the hitherto

unpublished collections in the Museum at Brno. The Palæolithic collections have lately been enriched by a beautiful new "Venus," which will be published shortly, as well as statuettes of horse and other animals. The beauty of the workmanship of many of the tools from Předmost and other localities is astonishing and shows a definite evolution from the Aurignacian, influenced to a certain extent by the Solutrean as well as by the Lower Magdalenian of France. Remains of mammoth and other Quaternary fauna abound in the loess.* But the diggings are not confined to loess deposits; a series of caves have been and are being dug in the limestone districts, and these have also yielded interesting finds, there being a poor but rather peculiar Mousterian at the base. M. C. BURKITT.

Religion.

Canney.

The Use of Sand in Magic and Religion. By Professor Maurice A.

6

Canney.

At Knutsford, in Cheshire, an old Spring Festival was revived some years ago at which, after a procession round the town, an elect maiden is enthroned and crowned Queen of the May. At this festival sand is strewn in front of many of the houses, and on it various designs are fashioned. The sanding is an old custom. It is known to have been practised at weddings at least a hundred and fifty years before the introduction of the Spring Festival. How much older the practice is it is difficult to discover, but it may be very ancient.

George Ormerod tells us that, according to the *Magna Britannia*, the sand strewn before the doors of the inhabitants on weddings and other joyful occasions was brown, and that the designs were made with white sand. He notes also that, according to another account, the choicest flowers of the season were strewn over the sanded pavement.†

We learn from Robert Holland that the most approved pattern for the sand-drawings resembled scale armour. Mention is made by other writers of true lovers' knots, hearts, and posies. Mottoes also were written on the sand. Mrs. Gaskell has recorded that, when she was married, all the houses in the town were sanded, and that two favourite verses written on the sand were:—

"Long may they live,
Happy may they be,
Blest with content,
And from misfortune free.

"Long may they live,
Happy may they be,
Blest with a numerous
Pro—ge—ny."

Robert Holland says that the sanding extends about half-way across the streets from each house, the patterns being made by trickling the sand through a large funnel.‡

In "The Countryman's Ramble" is preserved a song which describes the custom of sanding as follows:—

"Then the Lads and the Lasses their Tun-dishes handing,
Before all the doors for a Wedding were sanding,
I ask'd Nan to wed, and she answer'd with ease,
You may sand for my wedding whenever you please.'§

Writers on the history of Knutsford are at a loss to account for the origin and

* Dr. Absalon told me that the Museum possessed 10,000 mammoth teeth.

† "History of the County Palatine and City of Chester," 1882, vol. i, p. 80. Cp. Thomas Dugdale: "Curiosities of Great Britain," vol. vi, p. 1035.

‡ "A Glossary of Words used in the County of Chester," being vol. xvi of the *English Dialect Society*, 1886, pp. 298ff. Holland says that sometimes sands of various colours are used.

§ Henry Green: "Knutsford," 1859, p. 86; cp. Alfred Ingham: "Cheshire: its Traditions and History," 1920, p. 128.

significance of the custom. As to the origin, there are two legends, one of which is worth noting. It is recorded by P. H. Ditchfield. "King Canute forded a neighbouring brook, and sat down to shake the sand out of his shoes; while he was doing this a bridal party passed by, and he shook the sand in front of them, and wished them joy, and as many children as there were grains of sand."* This, like the other legend, assumes that the custom has been peculiar to Knutsford. This is not the case. William Henderson records the fact that at Newcastle-on-Tyne sand is strewn on the pavement before a bridal party tread on it.† In Sunderland, we are told, sea-sand was used at one time for the same purpose, though, later on, sawdust was substituted. A record of a sawdust wedding in Bewick's Lane, now called Huddleston Street, in the North Shore, is preserved by an "Old Correspondent" in the *Sunderland Herald*. "This ceremony consists in sprinkling the footpath of the street in which the bride lives, and along which she must pass in order to be married like a Christian at the Church. There is never any sawdust strewn for a 'broomstick coupling' at a register office."‡

We have found that sanding has been practised in a joyful ceremony. It is interesting to find that it has been practised in a mournful ceremony as well. It is found in connection with funerals. Writing of Bristol, J. F. Nicholls says: "There is also another curious local custom, confined, I believe, to one street only in this city. When an inhabitant of Back Street dies the whole surface of the street from end to end is covered with sand. Back Street was the site of King John's House, outside the walls of the borough, and since that date it may reasonably be supposed to have been a favoured residence of the Welsh, as it runs parallel to the quay known as the Welsh Back."§

Nicholls asks: Is the above a Welsh custom? Or is anything of the kind practised in Italy?

In Germany also sanding is practised in connection both with weddings and funerals. In an article on pre-historic graves in Germany—graves in which the skeletons were found to have been deposited on a layer of white sand—H. Busse writes as follows: "We see from this that it was a folk-custom on ceremonial occasions to use white sand, and that this custom dates back to the remote past—in the present instance to about 1,000 years before Christ. A survival of this custom may be observed daily even at the present time. In many villages of Mark Brandenburg I have found that at weddings the path from the bride's house to the church is strewn with white sand. So also at funerals the path to the grave, and even the pit itself, is strewn with white sand. I need mention only the Dorf Pechiele in Kreis Zauch-Belzig."|| Busse states that even in Berlin, when one makes a pilgrimage to the graves of one's dear ones at Friedhof, one finds that white sand has been strewn round graves.

Thus our attention is directed to the use of sand in connection with the dead and their disposal. Sand seems to have been used in burials in Britain. In an account of British and Saxon burial mounds of East Yorkshire, J. R. Mortimer says: "The use of material from a distance in building these barrows seems to have been practised over wide areas. Canon Atkinson, when opening barrows on the Cleveland moors, observed layers of white sand used in making the mound, which he considered was not obtainable within seven miles of the site of the barrows, and the greater part of another barrow was whinstone from a dyke three miles

* "Old English Customs," 1901, p. 195.

† "Folk-lore of the Northern Counties," 1879, p. 40.

‡ Everard Home Coleman: *Notes and Queries* for March 4, 1876.

§ *Folk-lore Record*, vol. iii, 1880, pp. 133f.

|| *Zeitschrift für Ethnologie*, 1903, p. 503.

"distant."* This fact, that sand seems to have been used in burials and to have been brought sometimes from a distance, may explain the dene-holes, which have presented such a puzzle to anthropologists; these may have been used either as burial chambers or as store-chambers for burial sand. The Rev. J. W. Hayes says, with reference to the deneholes at Bexley, that "at the bottom of every shaft, in all places where the deneholes abound, lies a cone of Thanet sand of far greater volume than can be accounted for by the mere attrition of the shaft side, or of the giving way of the mouth into a funnel shape." Hayes says that, according to W. H. Steadman's report, "these cones of sand usually contain three times as much sand, if not more, as could possibly have broken away from the shaft, and frequently reach almost up to the roof of the cavern, spreading out on all sides below to the depth of 6 to 8 feet and, at Abbey Wood, even more." Hayes thinks the only safe and logical opinion one can reach is that these shafts, after the chambers were emptied of their chalk and disused in consequence, became the receptacles of all the Thanet sand and loam excavated from newer shafts which the workmen poured down to get out of the way."† Evidently Hayes has not realised that sand often has a special significance. Had he done so, he would probably have reached a different conclusion.

Reginald A. Smith, also, has noted that the presence of sand on some sites where flints have been found is not readily intelligible to the archaeologist.‡

Sand was used in pre-historic burials in Scandinavia. Speaking of skeletons found in a tumulus at Stege, on the island of Møen, Sven Nilsson says that some of them were discovered sitting in a cell filled with sand, with amber beads round their necks. With reference to a tumulus on the Åsa-hög, near Quistofta, in the sepulchre of which were found a number of flint implements and ornaments of amber, Sven Nilsson notes as one remarkable circumstance that the sepulchral chamber was round, instead of oblong. He continues: "Another remarkable circumstance which we noted in the description of this sepulchre is that an older series of corpses were interred therein, without any regard to order or regularity, forming a layer, which was covered by a bed of sand forming a floor upon which other corpses had in their turn been deposited." In Scania, we are told, the Rev. M. Bruzelius examined a gallery-tomb and found in it, besides stone implements, clay urns, amber ornaments, "a vast quantity of human bones, divided into two layers by a bed of sand of about six inches in thickness."§

In an account of a Scythic tomb near the river Serebrjanka, in the Kiev district of Russia, E. H. Minns notes that the pit had been floored, and lined and covered with wood; that the wooden floor was strewn with white sand; and that the hole had been filled in with black earth.||

Let us go farther afield. Among the natives of the Florida peninsula, "the dead were buried in ordinary graves and in sand and earth mounds, and except ionally, in shell mounds, and here as elsewhere it was customary to deposit various utensils with the bodies."¶

Fritz Krause notes that at the present time the dead among the Pueblo Indians are buried in the sand or in clefts of the rocks, and that the grave is covered with sand, and often also with stones.**

* "British and Saxon Burial Mounds of East Yorkshire," 1905, pp. xxi f.

† "Deneholes and other Chalk Excavations: their Origin and Uses," in *The Journal of the Royal Anthropological Institute*, vol. xxxix, 1909, pp. 61 f.

‡ "Flint-finds in connection with Sand," *The Journal of the Royal Anthropological Institute*, vol. xlv, 1914, p. 384.

§ "The Primitive Inhabitants of Scandinavia," (3) 1868, pp. 128f, 131, 160.

|| "Scythians and Greeks," 1913, p. 175.

¶ "Report of the Bureau of American Ethnology," 1903, p. 119.

** "Die Pueblo-Indianer," 1907, p. 90.

In an account of a burial-room in Pueblo Bonito, George H. Pepper says that "the skeleton itself was resting on a layer of wood-ashes which had been spread on the levelled floor of yellow sand."* In another burial, "the floor had been covered with a layer of yellow sand on which a layer of wood-ashes had been placed."† In yet another many bodies "had been covered with sand as they were buried."‡

Among the Zoroastrians of the Kianian period (from c. 2000 to 700 B.C.), as a temporary expedient, "when a death occurred at a time when heavy snow, rain, or floods made the roads impassible, the Avestan people were enjoined to dig a furrow wide enough to admit the corpse, and a foot deep in hard ground, or half the height of a man, if the ground was soft, in a clean, dry, secluded part of the house. The corpse was to be laid in it, and the surface of the furrow was to be covered up with pieces of bricks or stones and with sand."§ This looks like a reversion, in cases of emergency, to an ancient practice of sand-burial. In India, according to A. Hillebrandt, when a Hindu feels the approach of death, he "must summon his relatives; hold friendly converse with them, and, if the dying-hour is very near, have himself placed on a cleansed spot on sandy soil."|| The bodies of Hindus are disposed of usually, not by burial, but by burning. Mrs. Sinclair Stevenson states, however, that when an ascetic dies, he is not burned, but buried. "He is placed in the grave in a sitting posture, and surrounded with salt and sand."¶

Of special interest is the use of sand in ancient Egypt in connection with the dead. In primitive times the body was simply laid in the sand, and, as Sir E. A. Wallis Budge says, the burial of the very poor must have been much the same in all times and in all dynasties. "The body, having been salted only, was laid in the sand to a depth of three or four feet, without ornament, and even without a coffin; sometimes even the salting was dispensed with."** When bodies were buried thus, whether deliberately or accidentally, it could not fail to be noted that the sand of Egypt had a wonderful power of preserving them. J. H. Breasted says that "the conditions of soil and climate in Egypt resulted in such a remarkable preservation of the human body as may be found under natural conditions nowhere else in the world."†† This phenomenon seems to have suggested that the use of sand in one way or another must be retained in the more elaborate burials and in ceremonies connected with the dead. In his account of the examination of the mummy of a Priestess of Amen, Elliot Smith says that when all the thoracic viscera except the heart had been removed, "it seems probable that the consecrated body and the separated organs were then placed in a bath of common salt (chloride of sodium) and left there for a considerable period, perhaps for one or two months. Ancient writers mention from 36 to 70 days."‡‡ For packing the body for mummification a mixture of earthy material (sand and mud) and sawdust, or sawdust alone, was used; and at a later stage in the wrapping of the corpse, the whole mummy was sprinkled with sawdust.§§ Further particulars are given in an

* "Putnam Anniversary Volume," 1909, p. 223.

† *Op. cit.*, p. 248.

‡ *Op. cit.*, p. 249.

§ Maneckji Nasservanji Dhalla: "Zoroastrian Civilisation," 1922, pp. 156f.

|| Hastings' "E.R.E.," iv, p. 476.

¶ "The Rites of the Twice Born," 1920, p. 200. ** "The Mummy," (2), 1894, pp. 315f.

†† "Development of Religion and Thought in Ancient Egypt," pp. 49f; *cp.* Sir E. A. Wallis Budge, *op. cit.*, pp. 315f; and G. Elliot Smith: "The Ancient Egyptians," 1923, pp. 31f.

‡‡ "An Account of the Mummy of a Priestess of Amen, supposed to be Ta-usert-em-Suten-pa," p. 161.

§§ *Op. cit.*, pp. 163, 171. The sawdust would contain resin, which was used in mummification. In his book, "The Migrations of Peoples," 1915, p. 115, Elliot Smith has noted the same use of sand among Indian tribes. When bodies were to be embalmed in Kentucky, they were dried and filled with fine sand.

account of the tomb of a certain Amenemhet by N. de G. Davies and Alan H. Gardiner.* In the wall-paintings of the tomb the mummy is depicted as a yellow form with a wig of blue. It stands, we are told, according to the traditional prescription, on a little raised mound of sand, which is coloured pink with red spots. The floor of the burial chamber was ceremonially sprinkled with sand, and on conclusion of the burial ceremonies it was brushed over to eliminate footprints.† There are two other curious references to sand in this connection. One of the duties which devolved upon the *shawabti*-figures, as servants of the dead, is said to have been that of "carrying the sand."‡ The instructions given to these *shawabti*-figures are recorded in Chapter VI of the "Book of the Dead," which is often inscribed upon them. An early version runs as follows: "O Shawabti, if the Osiris N be appointed "in the Netherworld to perform any of the tasks that are performed yonder even "as a man is bounden, namely, to cultivate the fields, to flood the meadows, or "to carry the sand of the East to the West, then speak thou, 'Here am I'."§ The other reference is associated also with Osiris. In the cxliind Chapter of the "Book of the Dead" one of the magical names of Osiris (and consequently of the dead man identified with the god) is "Osiris within his Sand," or, in some variants, "Osiris on his Sand." Again, the introductory words of Chapter clxxxiii describe the dead man as bowing before Osiris and exalting those "who are upon his sand."

Sand was used also in ancient Egypt in ceremonies connected with re-animation. Dr. A. M. Blackman tells us that at the ceremony known as the Opening of the Mouth, "before the commencement of the actual toilet, the officiant placed the "statue upon a mound of sand, generally depicted as an oval coloured pink with "red spots"; and that in the Abydos Daily Temple Liturgy the tenth episode is "Performing the pouring out of sand."|| Sand figures prominently again in a description of the funeral rites of Osiris in a long inscription of the Ptolemaic period. The inscription, which implies the resurrection of Osiris, is engraved on the walls of the god's temple at Dendera, a town of Upper Egypt. In the month of Choiak "small images of the god were moulded of sand or vegetable earth and corn, to "which incense was sometimes added; his face was painted yellow and his cheek- "bones green. These images were cast in a mould of pure gold, which represented "the god in the form of a mummy with the white crown of Egypt on his head."¶ Here sand is part of the divine substance of the god. At Busiris, "on the twentieth "of Choiak, sand and barley were put in the god's 'garden,' which appears to have "been a sort of large flower-pot."** On the thirtieth day of Choiak, "they repaired "to the holy sepulchre, a subterranean chamber over which appears to have grown "a clump of Persea-trees. Entering the vault by the western door, they laid the coffined effigy on a bed of sand in the chamber."††

[To be continued.]

REVIEWS.

Anthropology: Physical.

Keith.

The Antiquity of Man. By Sir Arthur Keith, M.D., D.Sc., LL.D., F.R.S.
Second edition. Two Vols. London: Williams and Norgate, Ltd.
Pp. xxxii + 753. Price 25s.

In this new edition, which has been awaited with interest, the author deals with the wealth of material relating to discoveries made since the first appearance

* "The Tomb of Amenemhet," 1915. p. 58. For the mound or bed of sand, *cp.* also G. Maspero, "New Light on Ancient Egypt," 1908, pp. 244f, 295.

† N. de G. Davies and Alan H. Gardiner, *op. cit.*, p. 93.

‡ Sir E. A. Wallis Budge, *op. cit.*, pp. 171f.

§ N. de G. Davies and Alan H. Gardiner, *op. cit.*, p. 93.

|| *The Journal of the Manchester Egyptian and Oriental Society*, 1920, p. 34.

¶ J. G. Frazer, "Adonis, Attis, Osiris" (2), 1907, p. 321. †† Frazer, *op. cit.*, p. 322.

of the work in 1914. As a necessary consequence the size has grown to two volumes. From the preface it appears that the author at one stage contemplated discarding the sections of a more technical character on the methods of reconstruction of skulls from cranial fragments, fortunately on second thoughts he left these untouched and has added sections of a similar character. Valuable as are the discussions on the archaeological and geological evidence of the age of the various specimens described, these details could be garnered from the literature, while the data on comparative anatomy could come only from one who has a great and historic collection at his disposal and who can utilise it in the true spirit of its distinguished founder.

The new discoveries to receive full treatment include paleolithic crania from Britain, France and Germany, remains from Malta, Rhodesian man and the later remains from Boskop and T'zitzikama in South Africa, the skulls from Wadjak in Java and Talgai in Queensland and the teeth of *Hesperopithecus* from Nebraska. Fresh notes have been added in practically every section remaining from the former edition.

The author is forced to conclude that evolution has been proceeding at a greater speed than he had previously thought, though in view of the magnitude of the changes he seems dissatisfied with the period of time as allowed by the more recent conclusions of the geologists as to the length of the pleistocene epoch: "I feel, as Huxley did when Lord Kelvin reduced the time limit at the disposal of evolutionists, that there must be a mistake somewhere." The author in the first edition drew attention to the types of surviving anthropoids, so similar to man that they must be collateral descendants of a common stock, and indicates that on this comparison it is reasonable to expect to find that in the past humanity was broken up into distinct genera each confined to a limited part of the earth with a tendency to produce varieties within each group. New discoveries have confirmed this; Rhodesian and Boskop types in Africa, Wadjak and Talgai types in Java and Australia. Migration he thinks has played only the most minor part in shaping the evolution of man, "we cannot account for the distribution of modern human racial types as seen at the dawn of history unless we presume that they have been evolved in or near the regions of the earth which they now occupy, or did occupy, at the beginning of historic times."

Recent evidence from other areas shows that if Galley Hill man did reach Europe in the early pleistocene the stay of modern types of man must have been short, and the author is less inclined to accept the evidence for this geological antiquity than formerly; still, as he is convinced that the pleistocene ancestor of modern man has not yet been found he has elected to leave the evidence as to the antiquity of the modern type as he set it out previously. There seems to be abundant evidence that the early development of the primitive forms was in the direction of increased size of brain wherever he developed, and many early specimens have cranial capacities as great as modern man. But somehow the big brained types did not always survive, a degeneration seems to have set in; for example, the large brained Boskop man can be traced through the cave remains on the south-east coast of Africa down to the small brained Bushman, some of whom offer the smallest capacities on record for normal specimens of mankind. Physique seems also to have decreased in this area and it may be that some relation between dietary and the secretions of the endocrine glands connected with growth will be shown to affect the nervous as well as the skeletal system. It is not certain, however, that the reduction from the very large skull capacities of the Boskop man was accompanied by a corresponding reduction in the number of the cerebral cells as the cavity may now be better filled and the convolutions more complex. Paying attention to the evidence of worked stones it seems that ancestors who had become human in that they were beginning to control their environment must have existed at an earlier period than any from which remains of bones have been found, the author still maintains his old conclusion

"there is not a single fact known to me which makes the existence of a human form
"in the miocene an impossibility."

F. C. S.

Assam : Ethnography.

Smith.

The Ao Naga Tribe of Assam. By Professor Wm. C. Smith, with an introduction by J. H. Hutton, C.I.E., D.Sc., Hon. Director of Ethnography, Assam. London : Macmillan & Co., 1925. Pp. xxvii, 244. Price 21s. net.

8

It is not often that we get a book by a writer who combines, as Prof. Smith does, the qualifications of a missionary and of a trained sociologist. The first half of this work contains an outline description of the Ao Naga tribe. This is adequate and accurate in the main, but some of the information is recognisable as being derived from Dr. Clark's "Dictionary of the Ao Naga Language," and here the writer has been incautious, for, scholarly though that work is as far as language is concerned, it contains statements of belief and tradition which are of more than doubtful accuracy. A long and interesting chapter is devoted to a discussion of the origin and affinities of the Aos. The writer admits that there is little evidence to support his theory that they are a backwash from the plains of Assam. The far more probable theory is that the tribe is of mixed origin and composed of immigrants who came through the Hills from the East. This is the theory discussed by Dr. Hutton in his valuable introduction. In dealing with the affinities of the Ao the writer confines himself to Indonesia, but they extend further than that, probably to New Zealand or even to South America ; on this subject, however, much research will be required before any certain conclusions can be arrived at. An important part of the book is the last two chapters. Here Prof. Smith discusses the effect on the Aos of their contact with Western culture, introduced to them unavoidably by Government and deliberately by the American Baptist Missionary Society. Here we listen to a sociologist with practical experience of mission work, and it is refreshing to hear a missionary discuss so frankly the mistakes of those with whom he has been associated. Even so I think he is inclined to minimise the harm for which they are solely responsible. He says, quite truly, that Government has removed an interest in life by the abolition of head-hunting. But both the Aos and other Naga tribes have survived this without serious detriment to themselves, for the official policy has been to leave untouched as much of their social life as possible. It is the missionaries who have brought about changes, with their encouragement of foreign dress, and their prohibition or discouragement of sacrifices, feasts of merit, dancing, singing, the wearing of old dress and the display of the insignia of wealth and valour. Even Prof. Smith admits (p. 8) that he tried to persuade a young Ao "to improve his general appearance by buying a pair of knee-trousers." I suspect that Prof. Smith now sees that it would have been no "improvement," and that there is no surer way of giving a savage a strange outlook than by dressing him up in strange clothes. It would be interesting if Prof. Smith would proceed to constructive work, and, taking the Aos as an example, give us his views of how a primitive people can best be trained to meet the dangerous impact of civilisation. Dr. Hutton's notes, in which he often differs from the writer, are a feature of the book, which touches on such broad questions that no anthropologist can afford to ignore it, whatever his special area may be. There is a good bibliography, but the index might have been fuller.

J. P. M.

Ethnology.

Haddon.

The Races of Man and their Distribution. By A. C. Haddon, Sc.D., F.R.S. New Edition. Cambridge at the University Press, 1924. 7½ by 5. Pp. viii + 184. Ten plates, diagram, bibliography. 6s.

9

Those of us who are engaged in teaching ethnology owe Dr. Haddon a great debt for the book he has produced. It summarises tersely but clearly just what

we wish our students to know on the subject and what many of us wish we could teach them. The arrangement is as follows, first a discussion of the classification of mankind, secondly—and this forms the body of the book—a detailed treatment by countries of various branches of mankind and finally a long summary and conclusion. The matter is all too brief but I understand that this is to a certain extent a summary of a larger book which Dr. Haddon has in hand. In a short review it is not possible to discuss the book as carefully as it deserves. I can only touch on one or two of the points which strike me as especially meriting discussion. The arrangement of the peoples of America, admittedly a difficult point, needs a further elaboration. In the scheme of classification the Palæo-Amerinds are grouped with the Indo-Afghans and the Nesiots, they are not even given a place in the index, and in the geographical section Dr. Haddon writes on p. 133 "This is certainly an old type of skull which has affinities with various skulls from the western Pacific and elsewhere, but this does not imply that there was any racial connection." Elsewhere he states that "we may regard it as representing an ancestral form of xanthoderms who had not acquired brachycephalism nor the more distinctive 'Mongoloid' character." I have chosen this example to shew how difficult it sometimes is for the student exactly to follow the line of argument, for I feel that in places Dr. Haddon has not quite linked up the various parts of the book. The student who knows little of the matter will read either the first part, or the second, or the third seriatim. If he is very diligent he may compare all three but he may sometimes feel a little bewildered as to the real relationships of the various peoples. Against this disadvantage he will find it easy to understand the actual population of any particular country and in the summary to see the case of environment versus race put in a way which none but a master hand could have accomplished. This part Dr. Haddon throws out as a challenge to all anthropologists. If I understand him correctly, he believes that it can be proved that certain features can be changed by environment but he does not see how this change can take place. He suggests that the most probable mechanism is natural selection by elimination. Some races have reached a specialised stage from which there is no turning back, whereas others are more "undifferentiated" and therefore more capable of progressive evolution. The question is a very difficult one and there are many points which need a further investigation. It seems probable that the application of the theories of modern genetics to ethnology may possibly be of service, but the effect, for instance, of the inbreeding which presumably is associated with certain forms of marriage, is not raised by Dr. Haddon. Space does not permit a further discussion of these points, in the meantime all we do is to hope that this book will prove to be the anthropological best seller it ought to be.

L. H. D. B.

Africa, East: Ethnography.

Browne.

The Vanishing Tribes of Kenya. By Major G. St. J. Orde Browne, O.B.E.
 London: Seeley, Service & Co., 1925. Pp. 284. Price 21s. net.

10

Major Orde Browne's book deals with the little known group of tribes situated on the slopes of Mt. Kenya between the Meru Akamba and Akikuyu. As would be expected, these people are considerably mixed; but the author considers the Chuka to form a distinct unit, upon whom more powerful neighbours have pressed from all sides. As a result of this pressure he suggests that Embu, Emberre, and Mwimbe tribes have split from the Chuka.

Throughout the book, both in the descriptions of customs and accounts of beliefs, the reader is continually reminded of the Masai, the Akamba, and Akikuyu. Thus the regulations observed with regard to eating meat resemble those of the Masai; the word for medicine man is Kikuyu; and of the eight kinds of *thahu* (cere-

monial uncleanness) given, six can be found among the Akikuyu, while numerous resemblances to the Akamba could be quoted.

There are, however, some sharp contrasts. The Chuka are said to have no belief in any kind of survival after death, and no trace of ancestor worship. Considering the importance of the influence of the dead for good and evil in Africa generally, and among the Akikuyu and Akamba in particular, it seems just possible that the author was led to this conclusion by the lack of mourning ceremony and of all care for the dead body that he observed among these people, and that further investigation might bring forth evidence in the opposite sense. Moreover, though the corpse is left to be devoured by hyenas, contact with it causes severe defilement; this scarcely points to the belief in complete cessation after death.

It is interesting to note that the pseudo-Mongolian type described in MAN, 1924, 130, 1925, 9, is found among the Chuka; also that a tendency to an irregular number of fingers and toes is not uncommon.

The whole book, though perhaps not so full of detail as some ethnologists would like, is marked by a sympathetic understanding of native problems and an enlightened attitude towards future development. It is undoubtedly worthy of a place in all African libraries, and is especially to be recommended to administrators and missionaries.

BRENDA Z. SELIGMAN.

Ethnography.

Joyce.

British Museum. A Handbook to the Ethnographical Collections. Second edition. By T. A. Joyce, assisted by H. J. Braunholtz, Trustees of the British Museum, 1925. Pp. xvi + 319. 8½ × 5½. 20 plates, 293 illustrations and 3 maps. Price 2s. 6d.

11

The second edition of this invaluable Handbook is as welcome as was the first. It contains some additional matter, the illustrations are more numerous, and, where necessary, the text has been brought up-to-date. We may remind our readers that the treatment is geographical, and that the book goes far beyond a technical description of the material objects in the Museum collections. It contains indeed, a summary of the cultural features that distinguish the chief divisions and subdivisions of the less advanced peoples of the world.

It is impossible to look over the illustrations in this Handbook without sighing for the advent of the day when the objects figured, and many thousands of others, now closely elbowing each other in the light—and shade—of exhibition galleries, or even more obscurely shelved in the gloom of the Museum vaults, shall be given a setting less unworthy of them, less discouraging to those in charge, and less disconcerting to those who seek to learn. Such a day may yet come, and some of us may live to see it. In the meantime we may congratulate our colleagues on the production of a guide that points out the way.

H. S. H.

Prehistory.

de Morgan, MacCurdy.

Prehistoric Man: a General Outline of Prehistory. By Jaques de Morgan, formerly Director of Antiquities in Egypt and Délégué-Général in Persia of the French Ministry of Public Instruction. Pp. xxiii + 304. 190 illustrations. London: Kegan Paul, Trench, Trübner & Co., Ltd. 1924. Price

12

Human Origins: a Manual of Prehistory. By George Grant MacCurdy, Ph.D., Research Associate in Prehistoric Anthropology with Professorial rank, Curator of Anthropology, Yale University, Director of the American School of Prehistoric Research in Europe. 2 vols. Pp. xviii + 440; xvi + 516; 254 + 410 illustrations. New York and London: D. Appleton and Company, 1924. Price 35s.

The first of these works is a translation, made by J. H. Paxton and V. C. C. Collum, of de Morgan's popular work "L'homme préhistorique." It is based upon

the author's "Les premières civilisations," published in 1909, and to some extent brought up to date with material from the works of Boule and Déchelette. It is essentially a popular work, and the standpoint is somewhat old-fashioned, as may be judged from its references to the "hiatus." It sets out to deal with the whole world and in particular the whole European region, but, though it describes fairly fully the archæological material from western Europe and the Baltic, from Greece, south-west Asia and Egypt, the only reference to archæological discoveries between the Rhine and the Caucasus is a very brief notice of the Hallstatt cemetery. The illustrations, though not new, are numerous and well selected, and the student may be glad to consult a clear summary of the author's work at Susa.

The second work is more ambitious and much more complete in its survey, for, though the author has not lived up to the announcement of his publishers that he was telling "the whole story of prehistoric man," he has told more than has hitherto been given in one work. The account of palæolithic man and his work is very clear and quite up to date, though it contains nothing which was not well known to students of this period. The later part, dealing with the Neolithic, Bronze and early Iron Ages, is not so perfect, for there have been no text-books to guide the author, though he has made good use of Déchelette. The one really useful item is the long appendix, giving a complete list, with full stratigraphical details, of all the palæolithic sites explored.

The book is a compilation, though an admirable one of its kind, well arranged and clearly written. It contains nothing new, not even the author's opinions on disputed questions. It is a text-book for students, extremely useful, but in no sense a contribution to knowledge.

H. J. E. P.

Britain: Archæology.

Crawford.

The Long Barrows of the Cotswolds. By O. G. S. Crawford, B.A., F.S.A., Archæology Officer, Ordnance Survey. Pp. xv + 246, 76 maps, plans and illustrations. Gloucester: John Bellows. 1925. Price 25s. net. **13**

Mr. Crawford has done excellent service to British archæology by producing the two "Professional Papers" on the Long Barrows and Megaliths of the Cotswolds and of south-east England; in this volume he has published in full the material which was only summarised in his first paper. Here, in addition to a valuable introduction, he has given all that is known from printed or manuscript sources, as well as from museum specimens, of the monuments in question. It is a useful record, to which have been added much information on folk-lore and a number of fine photographic views of the monuments. The volume is well got up and does both author and publisher great credit.

H. J. E. P.

CORRESPONDENCE.

Sudan: Ethnology.

Raglan.

To the Editor of MAN.

Tribes of the Southern Sudan.

SIR,—In a note to his Presidential Address, "Some little-known Tribes of the Southern Sudan," Professor Seligman mentions that I differ from him in holding that the Shiluk left the Nilotic "homeland" before the Dinka. It is with diffidence that I express an opinion differing even tentatively from that of such a recognised authority as Dr. Seligman, but my excuse must be that owing to the paucity of data the whole question is still very much in the air. **14**

The Upper Nile basin is inhabited by two races, one of dolichocephals and the other of mesaticephals, which differ radically from each other not only in skull shape but in other physical characteristics, as well as in culture and language. It is highly improbable that both originated in the same area, or have even lived for very long together in the same area, as in that case the proportion of intermediate types would be very much larger than appears to be the case. There are many indications that the distribution of the mesaticephals was formerly far wider than it is at present, and the inference is that they were the earlier inhabitants of the Nile Valley, in which case the dolichocephals must have originated elsewhere.

The dolichocephals are divided into two groups, the Dinka-Shiluk or riverain group and the Lotuko-Turkana-Masai or hill group, and taking the distribution of both groups as a whole, one is led to infer a centre of distribution to the south-east of the Nile Valley, possibly in the region of Lake Rudolph.

The riverain group of dolichocephals is, as far as the Nile Valley is concerned, of far greater importance than the hill group, and is divided into two branches, the Shiluk branch and the Dinka branch, to give the Arabic corruptions of their names by which they are generally known, and the question is, which left the "homeland" first?

Taking the present distribution, we find the Dinkas (including the Nuers, who are the same people) occupying a central position with a boundary which can be drawn with a single continuous line. The Shiluk-speaking people, on the other hand, are divided into four (or more) isolated tribes, the Shiluk proper, the Anuak, the so-called Jur of the Bahr-el-Ghazal, and the mesaticephalic Acholi. There are then two alternatives, either the Dinka came first and the Shiluk afterwards spread themselves in a circle round them, or the Shiluk first occupied the whole of what is now the Dinka-Shiluk area, and were subsequently driven out of a great part of it by the Dinkas. I prefer the latter theory, and am inclined to suppose that the Shiluks came *via* the Pibor, dispossessed the inhabitants of the Nile-Sobat region, and gradually spread up the Nile. Later came the Dinkas, moving in a north-westerly direction, and struck the Nile near Bor. The Shiluk south of this line were driven into the mountains, and succeeded in imposing themselves and their language on the mesaticephals now called Acholi, who had then a wider distribution (*cf.* the History of Wales).

After driving the Shiluks of the West Nile bank into the interior, the Dinkas drove the mesaticephals out of the cattle-bearing part of the Bahr-el-Ghazal. They were long held up by the Shiluks on the line of the Sobat, but eventually broke through, thus separating the Shiluk from the Anuak.

The above narrative is, of course, purely hypothetical, but it does, I venture to think, account for the facts better than the contrary hypothesis.

I will not trespass further on your space by dealing with the hill dolichocephals, but would say that some confusion has been caused by speaking of "the Bari-Masai group of languages," as if Bari were a pure dolichocephalic language. As a fact it is a hybrid language, in which mesaticephalic elements probably predominate.

Yours faithfully,

RAGLAN.

Wishaw House, Wishaw,

1st November 1923.

Magic.

Thomas.

To the Editor of MAN.

The Significance of Colour in Ancient and Mediæval Magic.

SIR,—With reference to Mr. Wilfrid Bonser's interesting article in MAN, 1925, 118, I would offer the following remarks.

Although Mr. Bonser explicitly mentions the sympathetic factor of colour in magic, and quotes instances in his article, he does not seem to have carried research

on these lines far enough, and to carry them a little further we may leave aside the question whether "magicians" in general recognised any inherent magical or psychic properties of colours *qua* colours, and here only consider black—widely acknowledged as symbolical of evil and gloom.

The devil threw the bell into the well, and if we suppose that his powers were invoked to get it out again, the reason why the oxen had to be *entirely* black is obvious. The Apis bull and the animal for the Jewish sacrifice had to be without spot, entirely of one colour. This is clearly symbolical of perfection and has nothing to do with the colour.

There seems to be a connection between this perfection and a child to be brought to maturity,

Blood is known to be an attraction to gods and demons of every kind: *e.g.*, the Arab *afrit* is attracted by spilt blood.

Hence red things—berries, strings—hung and tied *outside* to keep or attract evil beings *outside*, and red water to represent blood. It is obvious why the cloth for curing fever is *red* and why it is kept in the shade.

Mr. Bonser is incorrect in saying that the gods of Ancient Egypt wore self-coloured garments. Osiris is sometimes arrayed in a red loincloth with blue-centred white spots, and Nephthis often wears a green and white wing-costume. Isis wears at times a red garb with white ornament, and a blue sash. These are XVIIIth Dynasty examples.

In conclusion, it is probable that were, in every case, the nature of the disease cured known, and in general the kind of magic to be performed with objects of different, simple or variegated colours, then some sympathetic connection between the colour and the thing to be magically wrought upon, or the god or demon invoked, would be revealed, and Mr. Bonser's article changed to an interesting paper on sympathetic magic. This is rendered more probable because the colour sense is so rudimentary among many more primitive peoples, who recognise similarities in shape and hue and appearance and therefrom establish magical connections.

Yours faithfully,

ERNEST S. THOMAS.

Pitt Rivers Museum,
Oxford.

ANTHROPOLOGICAL NOTES.

The Library of the Royal Anthropological Institute.—The Council, having had placed at its disposal certain funds, of which particulars will be given in the Report of Council to be presented at the forthcoming anniversary meeting, has decided to obtain the services of a skilled librarian for a period, in the first instance at any rate, of some three to four years. The appointment has been offered to, and accepted by, Mr. V. Gordon Childe. Mr. Childe began work on 1st December. 16

As a number of Fellows have expressed their desire that the Library should be open at times in the evening, owing to their inability to avail themselves of the facilities of the Reading Room and the Library during the day, it has been decided that, as from 1st January, the Library shall be open on two evenings in each week, on Tuesdays and Thursdays, from 5 p.m. to 8 p.m. Should the experiment prove successful, and should there be any further demand, it may be possible to increase the number of evenings in each week on which the Library will be open.

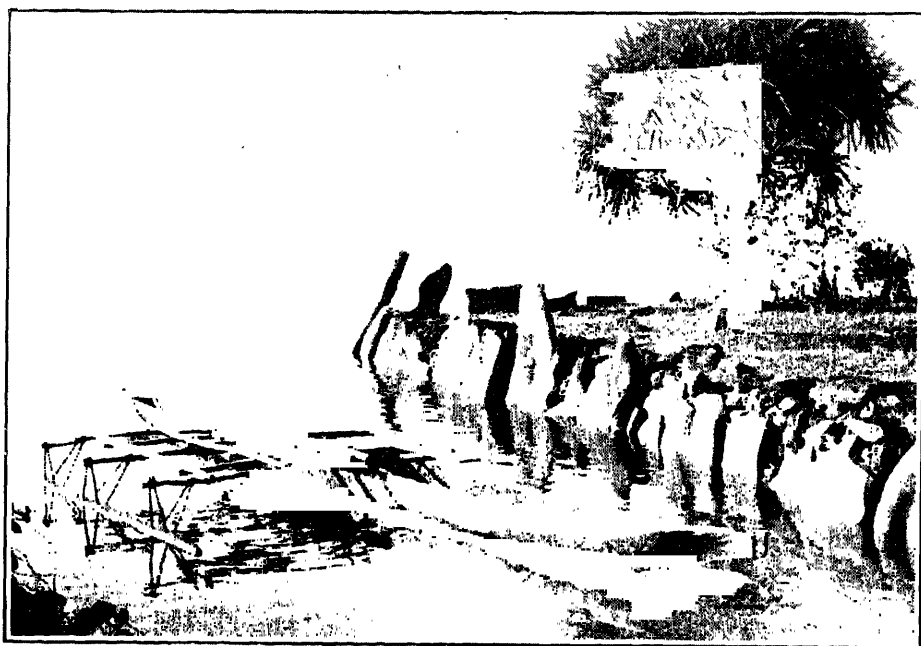


FIG. 1.—NORTH SIDE OF UTHUI NAMBOU, MBAU (FIJI).



FIG. 2.—RUINED "WHARF" AT MUA (TONGATABU).

THE MEGALITHIC SEA WORKS AND TEMPLE PLATFORMS AT MBAU IN FIJI.

ORIGINAL ARTICLES.

With Plate B.

Fiji: Technology.

Hornell.

The Megalithic Sea Works and Temple Platforms at Mbau in Fiji. (*Percy Sladen Trust Expedition, 1924-5*). By James Hornell, F.L.S., with Plate B.

17

Mbau is the most historic place in Fiji; it was the seat of government of King Thakombau, who brought under subjection many of the other chiefs in the group by the middle of last century. It is a small island, approximately twenty-two acres in extent, lying half a mile off the eastern coast of Viti Levu, and about eighteen miles north-east of Suva. A considerable part consists of a bluff, some 100 feet high, occupied to-day by missionary buildings and the Royal Cemetery of the Thakombau family, chiefs of Mbau. The rest of the isle is low-lying, composed partly of levelled talus from the crumbling strata of the bluff and partly of land reclaimed from the shallows of the encircling waters. This latter is protected directly from the inroads of the sea by a facing either of stones or of rude piling, the barrier reef fronting the coastline of Viti Levu giving such shelter from ordinary storms that comparatively weak defences suffice for the purpose. The present state of these sea-works partakes of the decay characteristic of the arts and crafts of the South Seas since the coming of the Whites; enough, however, survives of the old works in this island to enable us to reconstruct their original condition with certainty.

The thatched houses of the inhabitants are scattered without order over the low-lying ground. In the centre, at one end of what was the *rara* or public assembly ground, stands a Wesleyan Methodist church, solidly built of stone, *vis-à-vis*, at the other end of the *rara*, to the two-decker rectangular mound, faced with megalithic slabs, whereon the principal temple stood in pre-Christian days. Nearby the double platform of a smaller temple, that of the Lasakau people, also survives. These and the sea-works are what I am now concerned to describe.

The Mbau sea-works lie almost entirely on the seaward sides of the island, due probably to the greater ease of landing there at all states of the tide; the strait on the landward (southern) side is so shallow at low tide that it can be crossed by wading. It is probable that some low-lying land must have existed on the seaward side at the date of the original settlement.

The sea-wall or embankment is broken at irregular intervals by parallel-sided little inlets or "docks," into which the big outriggers and double-canoes were run. The outrigger canoes of to-day, degenerate in size, still make use of them. The two sides are protected from falling in by the same means as is used on the sea front. At the inner end is a sloping beach or ramp, up which the canoes are dragged when not in use. It follows, therefore, that along the whole sea-front rectangular wharf-like "piers" (*uthi*) alternate with little docks. At present there are 25 of the latter, of varying length and breadth. A plan of the island, made in 1880, shows only eighteen docks; with the disappearance of the large canoes of last century, it would seem that, following upon the ruin of some of the older wharves, a number have been subdivided and rebuilt in the makeshift manner of many now existing. Reference to Fig. 1 will make the above clear and save much wordy explanation.

Three methods of protecting the sides of the "wharves" are seen. The first and most characteristic is by means of large slabs of rock set on edge, of varying sizes and shapes, running to 11 feet 8 inches in length (height). The actual height of the sea-wall above the sea bottom is only 6 to 7 feet, so the longer slabs project well above the surface level of the wharves. The finest and most conspicuous

slab-faced *uthi* is *Uthi nambou*, stretching out from the phallic-like pillar which formed the installation stone of Rata Thakombau, when proclaimed Chief of Mbau by his Lasakauan followers.* (Fig. 2.)

Uthui nambou is 80 feet long on the south side, 88 feet along the north, and 44 feet wide. In its length it surpasses all other *uthui*, projecting into the sea

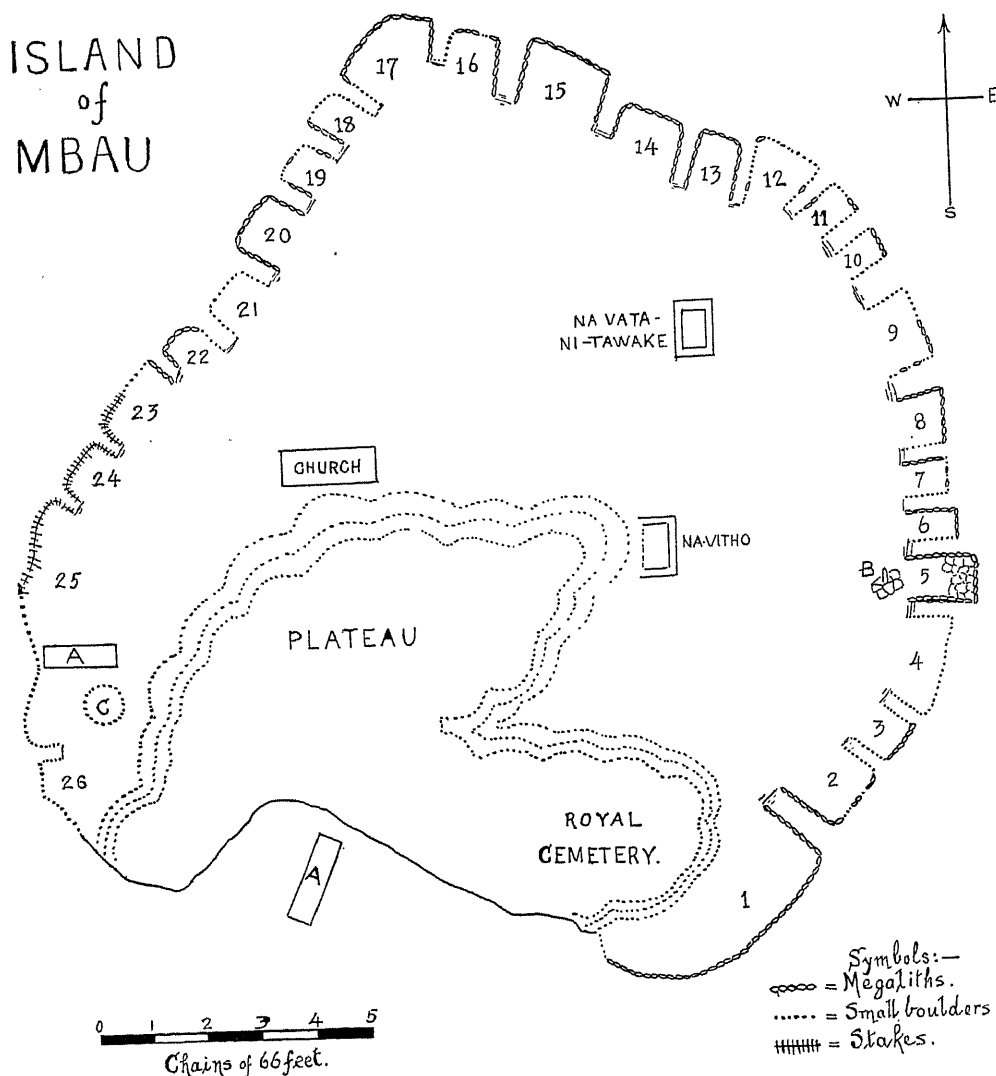


FIG. 1.—SKETCH PLAN OF MBAU.

considerably beyond its neighbours, thus emphasising its Royal supremacy. The slab revetment is nearly complete along all three sides (Pl. B, Fig. 1) and a con-

* From this fact and a natural reluctance to refer to evidences of former cannibalistic practices, the official name of the stone as stated by Ratu Pope is *Vatu-ni-vei-buli* ("Installation stone"). Originally, however, the name was given to me by a Mbauan as *Vatu-ni-bokola*. Now *bokola* means the human victim brought to be cooked and eaten, so *Vatu-ni-bokola* would seem to signify the stone against which the *bokola* was brained. On the other hand the phallic shape suggests that it may have been a sacred pillar—the abode of a god—such as Williams figures in his *Fiji and the Fijians*.

siderable portion of the horizontal or land surface is flagged with fairly large slabs. Those protecting the seaward face of this *uthui* comprise some of the largest and longest of any to be seen in Mbau. Eleven slabs suffice to protect the sea frontage of 44 feet, giving, therefore, an average width of 4 feet. Specially large ones are set

at the corners; that at the north corner, set obliquely facing roughly N.E., is 11 feet 8 inches long, 4 feet 8 inches wide for the greater part of the length, with an average thickness of 8 ins.; in outline it is roughly rectangular.

Next it is an irregularly shaped round edged slab, roughly crutch-shaped, varying from 2 feet 6 inches to 5 feet 4 inches in width. At the opposite corner, but forming in reality the outermost of the side-wall slabs on the south side, is a huge sub-triangular water-worn slab 9 feet 10 inches high, with an extreme width at the base of 5 feet 8 inches and a thickness varying from 5 to 7 inches. The others along the sea-face vary from 7 feet 9 inches to 8 feet 4 inches in length, and the thickness from 4 to 11 inches. The upper end of one is broken off and another lies prone in the mud. They are not embedded to any extent in the sea-bottom, save where silting has occurred; they incline slightly inwards and are backed by rough boulders of reef-rock, often coral, with sand and gravel filling behind. The slabs themselves are generally of a coarse gritty structure and are reported to have been brought from Sawakasa, Nakalawatha and other places on the coast of Taitu, the district of Viti Levu lying north of Mbau. Many are of gritty beach sandstone, but others, usually of finer grain and more homogeneous, have been derived by denudation from hard flaggy layers of tufa in the adjacent cliffs. Little or no attempt has been made to shape them; the builders have been content to use them of the original shape as quarried, making good the irregularities partly by a clever fitting together of complementary irregularities in adjoining slabs and partly by filling in gaps with coral boulders.

Uthui nambou must have been the most important *uthui* on the island, judging from the size of the blocks used, the flagging still existing over part of its land surface, and the presence of the *vatu-ni-vei-buli* (or *vatu-ni-bokola*). Originally it was flagged over its whole horizontal surface, for not only does a large portion of flagging remain at the seaward end, but flag-slabs also surround the Installation Stone. There can be little doubt that it was the ceremonial landing place of the head of the dominant community—the superiority of its size and construction and its association with the installation stone, all go to prove this. Probably it was where the raiding canoes of the Lasakau warriors landed the victims required for sacrifice or the feasting of distinguished visitors, and possibly where they were brained if yet alive.



FIG. 2.—THE INSTALLATION STONE, *Vatu-ni-vei-buli*.

The second and third methods of revetment are comparatively modern attempts made to repair *uthui* which had fallen into ruin. One is to build up the wall with small reef boulders, 9 to 12 inches in diameter, incorporating fragments of the older slab-facing when convenient; the other, practised by the poorest of the three island communities, the Soso fishermen, is a makeshift and temporary palisade of stakes. That both these are modern degeneracies is certain. In most cases where loose boulders are used, occasional slabs, sometimes entire, more often fragmentary, are incorporated in the revetment; additional evidence is furnished by a sketch made in 1868 of the double-canoe shed that formerly existed adjacent to the present bathing pool at the Soso end of the Island.*

The whole sea front here, save the ramp into the shed, is shown as slab-faced; to-day the shed is gone and no slabs are to be seen in the present sea-front, which consists entirely of modern decadent work, for the most part made up of small coral or reef boulders, the remainder protected by a palisade of stakes.

The following is a list of the 26 existing *uthi*, with their approximate dimensions. In it I omit the prefix *Uthi*, meaning "promontory" or "projection," for the sake of brevity.

(NOTE.—The "docks" between *uthuis* numbered 2 and 3, 3 and 4, 10 and 11, 11 and 12, 20 and 21, 24 and 25, and 25 and 26 are presumably modern, if the 1880 plan on which text-figure 1 is based be reliable in this respect, as these "docks" are not shown thereon.)

Name of Uthui.	Ownership.	Length of	
		Frontage.	Length of Sides.
1. Mataiwelangi - - -	Mbau -	350 feet.	40 and 110 feet.
2. Mathuwata - - -	" -	76 "	80 " 46 "
3. Totoya - - -	Lasakau -	44 "	48 " 38 "
4. Kaloka levu - - -	" -	120 "	47 " 56 "
5. Nambou - - -	" -	44 "	80 " 88 "
6. Nathokula - - -	" -	28 "	70 " 70 "
7. Vatu naua - - -	" -	34 "	54 " 48 "
8. Mataravoka - - -	" -	52 "	54 " 50 "
9. Nairotu - - -	" -	70 "	54 " 40 "
10. Naisongombiyau - - -	" -	28 "	42 " 50 "
11. Navo lautoka - - -	" -	42 "	50 " 54 "
12. Levuka - - -	Mbau -	62 "	58 " 96 "
13. Na-vesi-kalakala - - -	" -	56 "	64 " 80 "
14. Sinu - - -	" -	62 "	70 " 38 "
15. Nanduru vesi - - -	" -	90 "	56 " 76 "
16. Naua - - -	" -	70 "	36 " 48 "
17. Muaindule - - -	" -	62 "	48 " 62 "
18. Wangga talatha - - -	" -	44 "	58 " 52 "
19. Muai silana - - -	" -	36 "	46 " 54 "
20. Nambaubau - - -	" -	60 "	57 " 46 "
21. Nai Kasakasa - - -	" -	48 "	44 " 58 "
22. Nukurua - - -	" -	48 "	38 " 26 "
23. Na tuaika - - -	" -	76 "	34 " 25 "
24. Nalovo - - -	Soso -	70 "	24 " 22 "
25. Naisasavitu - - -	" -	236 "	19 " 30 "
26. Nakambula - - -	" -	80 "	24 " —

* This will be reproduced, I hope, in a work dealing with the outriggers and double-canoes of Polynesia which is in preparation.

Notes on the foregoing.

1. Uthui Mataiwelangi.—This is a long curved plot of land at the extreme south end. Except by water, it cannot be reached save at the north end, so in the turbulent days, when he was fighting for supremacy, Thakombau moved his residence here, to obtain both regal privacy and to secure himself more easily against plotters. He extended the area and repaired the sea-wall. This land is now occupied by his grandson, Ratu Pope. The sea wall is in good repair.

2. Uthui Mathuwata.—In ruin; only a few slabs of the old wall remain.

3. Uthui Totoya.—The sea-front of slabs is in fair condition.

4. Uthui Kaloka levu.—Revetment modern, of reef boulders. One of the longest of these "wharves."

5. Uthui Nambou.—This, the most conspicuous and far projecting *uthui*, has been fully described above.

6-26. The condition of the revetment in these varies greatly. The slab frontage of Uthui Nathokula, U. Naisongombiyau, U. Na-vesi-kalakala, U. Sinu, U. Nanduru vesi, U. Naua, U. Nambaubau, and U. Nukurua, is in fair condition; but in the others it is either incomplete or entirely replaced by coral boulder revetment. Sometimes one or both of the sides of the *uthui* are in good condition, while the front is ruined, or it may be *vice versa*. Uthui Na-vesi-kalakala is conspicuous for the height to which three of the slabs in the front wall rise above the ground level. The plan given of Mbau shows the respective condition of these *uthui* diagrammatically, each form of revetment being shown by a separate symbol.

On Uthui Nanduru-vesi, distinguished by the height of several slabs on the sea front, was the chief house of Thakombau before he moved to Mataiwelangi. Similarly, on Uthui Muaindule was the residence of his father, Ratu Tanoa Visawangga, who died in 1852.

23. Uthui Na tuaika.—Half the frontage (the south part) has a revetment of coral boulders, the other half is partly of coral stone reinforced by a stake palisade. This is a canoe-building area, the hinterland occupied by the carpenter class who make canoes.

The remainder of the sea-front is in the possession of the Soso community of fishermen. The old slab frontage, shown in the 1868 sketch, has entirely disappeared and has been replaced partly by a rough coral stone revetment, partly by palisading.

A small tongue-shaped area of low land was formerly revetted with slabs in the bay on the south side of the island. On it was built one of the two great canoe sheds used to house the largest of the Chief's double-canoes (*Wangga tambu* [= *tapu*]). To-day all trace of the shed and of the slabs that faced the reclaimed land have gone—the latter probably broken up to help to form the drains which now intersect the town area.

Another shed, as already noted, existed as late as 1868 on the wide stretch of ground known as Uthui Naisasavitu. These sheds were huge bamboo-framed structures, thickly thatched, the beams and rafters ornamented with coloured sinnet, black, red and yellow, in geometrical designs, made by winding the sinnet around the beams and poles. They measured at least 100 feet in length by a height and width of some 25 feet. That on the Soso ground dwarfed all else by its huge proportions.

It is fortunate that Thakombau repaired and extended the largest of the *uthui* during his reign, and that the method employed to transport the slabs is still remembered. There is no reason to doubt that this method did not differ from that employed in much older days. According to Ratu Pope, the stones, selected and loosened from their bed between tide-marks, were slung beneath large double-canoes by ropes passed around the stout booms that cross and connect the hulls. The work was done at low tide; when the water rose the canoes acted as pontoons

and raised the stones from the bottom. Several days were spent on the journey to Mbau, for it was impossible to sail or scull the canoes—they had to be poled all the way, work too that could only be done towards high water of each tide. Tradition says that these revetted “wharves” were first constructed by the original inhabitants, the Levuka tribe, whom the Mbauans expelled when they took possession of the island.

The little docks that separate the *uthui* from one another vary considerably in length according to the lengths of the bounding *uthui*. In width they are more regular. Eight are 18 feet wide; a few (six) run rather less, 15 and 16 feet, while a larger number (seven) are between 20 and 24 feet. The one between Uthui Mataiwalangi and Uthui Mathuata is still greater, being 26 feet wide, and those on either side of Uthui Nairotu are respectively 30 and 44 feet wide. As we know, another wide one (used for a double-canoe) formerly existed on Uthui Naisasavitu. The width of these “docks” is governed by the size of the canoes which make use of them. Ratu Pope’s dock, 26 feet wide, is of this width to accommodate his large *thamakarau* (outrigger canoe). It is obvious that the majority of canoes are much smaller, as a width of 18 and even 15 feet is enough for the majority in use now.

The other megalithic remains are those forming the retaining walls of the two double platforms whereon the two principal temples of Mbau formerly stood. The larger of these, the one facing the Wesleyan Methodist Church, consists of a smaller rectangular platform superimposed upon a larger one. The latter measures 59 by 45 feet, the former 47 by 35 feet. The height of the lower is 6 feet, that of the upper $3\frac{1}{2}$ feet. The width of the “step” around the base of the upper platform is very irregular, ranging between 4 feet at the south end and 8 feet at the north end.

The north and the west walls are each broken by a flight of steps leading to the summit, hence the east and south walls are the better adapted for study, and particularly the former. On this side the number of megaliths in the wall of the lower terrace is 15, without counting two fragments of basaltic columns at the south-east corner, put in probably during repairs. The length being 59 feet, this gives an average width of almost 4 feet to each block, precisely the same as that of the blocks facing Uthui Nambou. The lengths of the slabs are also much the same, varying from 5 feet 7 inches to 9 feet above the ground level. A little must be added to this for the amount sunk below the surface of the land. Figure 3 is a

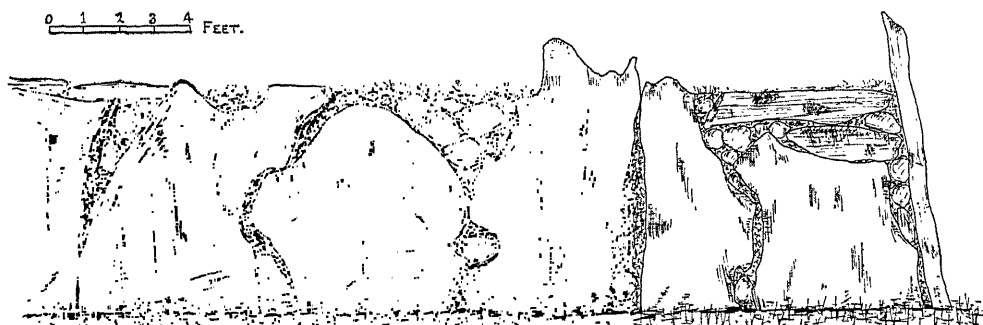


FIG. 3.—MEGALITHIC SLABS IN THE NORTHERN HALF OF THE EAST WALL OF THE LOWER PLATFORM OF THE PRINCIPAL MBAUAN TEMPLE, *Navata-ni-tavake*.

rough sketch of the northern half of the east side of the lower terrace. The method of construction is identical with that of the seawalls; the slabs are set on end inclined slightly inwards, backed by a buttress wall of coral blocks, with the central space filled in with gravel and earth.

The steps on the west and north sides appear of modern construction, as they are formed with small squared coral blocks. This inference is confirmed on reference to a plate in Vol. II of "Fiji and the Fijians," where this temple is figured; in this view the entrance is depicted as by means of a notched plank or tree trunk and not by masonry steps of any kind. An interesting feature to-day is the presence of five broken basaltic columns, spoiled from the Rewans, who are said to have, in turn, captured them from the people of Lakemba. These are not shown in the plate referred to above (published 1858); the artist, it should be noticed, has drawn the megalithic facing much too regular; the slabs are essentially uneven and without dressing.

The second set of temple platforms is built at the foot of the cliff, south of the east end of the *Rara*. A small Government office now stands thereon. The length of the upper platform is about 58 feet, that of the lower is indeterminable, as the ends are destroyed. Allowing $4\frac{1}{2}$ feet at each end this would make it 67 feet, a length considerably greater than that of the other temple. But in width it was much smaller, for, though the back walls are gone, it is clear that the width of the lower platform must have been about 28 feet, and certainly not more than 30 feet. The height of the lower platform to the earth filling is, roughly, 3 feet; that of the upper, about 4 feet. Several of the facing slabs project above the filling level, the largest, the one set obliquely at the south-east corner, being 8 feet 1 inch above the ground.

Only one flight of steps occurs, situated at the centre of the mid-length on the east side. Unlike those of the other temple, each step consists of a single slab, 12 in all, flanked on each side by a sloping rail made of upright slabs of uniform height.

This temple was termed *Navitho*; it belonged to the Lasakau community, whereas the greater one facing the church was the property of the Mbauans. Here it may be well to mention that three separate communities or tribes occupy the island. First the Mbauans, who form the aristocratic section, the conquerors of the island from the original occupants, the Levukans, who after expulsion settled in Lakemba; next the Lasakau community, fishers of men, who were the henchmen of the Mbauans, raiding for human victims, fighting when necessary, and fishing in times of peace with nets and wicker traps. Last of all the Soso people, lowest in the scale, who earn a living by tending the fish corrals or pounds so numerous along this coast, and by cultivating land belonging to the Mbauans on the adjacent mainland.

As to the age of these remains, it is difficult to obtain any definite evidence. Tradition says they made them thus from time immemorial. A ray of light comes from Tongatabu. There at Mua (Lapaha), the seat of the Tui Tongas from about the thirteenth century, I saw a megalith-faced "wharf" precisely similar to those of Mbau (Pl. B, Fig. 2), together with scattered slab megaliths, some still upright, that furnished evidence that in its day the shore line of Mua had been broken into a series of "wharves" and "docks" on the same plan as those of Mbau, the sole difference being that many of the megaliths of Mua were considerably larger in dimensions. Now we know from tradition that the sons of the great Tongan king, Tui-ta-tui, settled at Mua, so far as we can deduce from dates founded on the study of genealogies, somewhere towards the close of the thirteenth century. We are also told that their reason for moving to Mua from Heketa was that it afforded a more sheltered and commodious harbour for their great canoes, these chiefs being particularly fond of sea-adventure.

What makes it probable that even at this early date (thirteenth century) megalithic sea-works were carried on at Mua is that tradition associates these chiefs with the erection of the Ha'amonga trilithon at Heketa, though another

attributes this great work to their father. However this be, it is clear that these chiefs, the founders of Mua as a harbour and seat of government, were well acquainted with megalithic engineering. To the men who must have seen the erection of the great trilithon, the erection of megalithic faced "wharves" and "docks" as at Mbau would be child's play. Further, the evidence of the wonderful *langis*, three-step truncate pyramid tombs, scattered around Mua harbour, some dating from this period, further attest the predilection of the Tongan Supreme Chiefs for the erection of megalithic works. In one of these *langis* I measured a corner stone 21 feet 6 inches in length on one face, 6 feet on the other, with a height above ground of 3 feet and a thickness of 3 feet 4 inches (minimum).

It is a significant fact that the rectangular temple platforms of Mbau are built on the same plan as the Tongan *langis*; they have also a strong family resemblance to the double platforms of the "Club-houses" of Yap; indeed these latter are suggestive in themselves of the Fijian temple superstructure as seen in models and the illustrations of Williams.

EXPLANATION OF ILLUSTRATIONS.

Plate B.

Fig. 1. View of the megalithic slabs forming the face of the north wall of Uthui nambou; an outrigger canoe lies "in dock" alongside. Mbau Island, Fiji.

Fig. 2. Ruined megalithic revetment of a "wharf" (= Fijian *uthui*) projecting into the sea at Mua in Tongatabu. (Photographed by J. Hornell.)

Text Figures.

Fig. 1. Sketch plan of Mbau Island, Fiji, to show the arrangement and present condition of the wharves and docks along the sea-front. (Based upon a Government survey of 1880, kindly supplied by the Survey Department, supplemented by measurements made by the author).

A, A. The sites of two double-canoe sheds.

B. The "Installation Stone," *Vatu-ni-vei-buli*.

C. Public bathing pool.

Fig. 2. The so-called "Installation Stone" (*Vatu-ni-vei-buli*) of Ratu Thakombau, with remains of massive pavement around.

Fig. 3. The megalithic slabs forming the northern half of the east wall of the lower platform of what was the principal Mbauan temple, *Navata-ni-tawake*. From a sketch made in 1925.

Religion.

Canney.

The Use of Sand in Magic and Religion. By Professor Maurice A. Canney. Continued from MAN, 1926, 6, **18**

We have found that in connection with funerals the use of sand has been fairly widespread. In connection with weddings it has not been so common. Here, however, we have to note that it is a widespread custom to strew or sprinkle at weddings or in connection with weddings other substances, some of which resemble sand and may have the same significance. Edward Westermarck observes that "the custom of throwing grain, seeds, or dried fruit of one sort or another over the bride, or over the bridegroom as well, or sometimes over other persons present at the wedding, has been observed from India to the Atlantic Ocean."* W. Crooke states that at Cranbrook in Kent, when a newly-married pair leave the church, the path is strewn with what he calls "emblems of the bridegroom's calling." Thus, "carpenters walk on shavings, butchers on sheepskins, shoemakers on leather parings, and blacksmiths on scraps of old iron."† John Brand‡ notes that in England there was "a custom at marriages of strewing herbs and flowers, and also rushes, from

* E. Westermarck, "The History of Human Marriage," ed. 5, 1921, vol. ii, p. 471.

† "Folk-Lore," vol. xiii, pp. 245f.

‡ "Observations on Popular Antiquities," 1888, pp. 364f.

"the house or houses where persons betrothed resided to the church." In Morocco the grit of wheat is used. Among the Ait Yusi it is thrown on the floor of the bridegroom's tent or house; and at Amzmüz when the bride is carried into the house of the bridegroom, the bridegroom, who is sitting on the roof, throws on her grit removed from the wheat on the day when it was cleaned.* In India red powder is used in much the same way at the Hoti festival. The Hoti is a Spring Festival. As soon as the bonfire is lighted, children proceed to throw dust and red powder over each other and over some of their elders. "This powder-throwing is continued throughout the next day, and by Kotis it is kept up for at least three days. Children are careful never to bespatter their maternal or paternal aunts, but they specially try to throw it over the wives of their elder brothers, wishing them each a son during the coming year." Here red powder is clearly a fertility substance. "Indeed," says Mrs. Sinclair Stevenson, "so connected is Hoti with fertility rites, that if more than a year has passed since the wedding, and a wife has no child, she and her husband often walk round the fire with their garments tied together and offer a coco-nut to it, and the children take special care to sprinkle such a couple the next day." Both men and boys also throw the powder over one another, wishing the recipient good luck for the year.†

Turning now from the use of sand in funerals and weddings to its use in other connections, we have noted incidentally the use of a bed or mound or platform of sand among the ancient Egyptians. A similar use is found in Australia, North America, and India. In Central Australia a sand mound and sand drawings play an important part in the ceremonies concerned with the Wollanga totem of the Warramunga tribe.‡ In North America, to take one out of many examples, mounds and ridges of sand figure before the Flute altars at Mishongbnoir. In front of the reredos of the Macileñya two figurines were set on small heaps of sand—one on the right called the Flute youth; the other on the left, the Flute maid.§ In India, Bishop Henry Whitehead found an interesting example of the use of a platform of sand. At Trungalur, in the Trichinopoly district, he saw a small enclosure sacred to Kurumbaiamma. During a festival a small *pandal* (i.e., booth) is erected in the enclosure. Under this is placed, to represent the goddess, a small earthen pot, curiously decorated. The pot is filled with water. In the mouth of it are put some coco-nut and oleander flowers, surrounded by a sheaf of mango leaves. A pointed stick of bamboo, with a lime stuck on the end of it and a small silver umbrella also figure among the decorations. "This decorated pot is placed on a small platform of sand, and about eight measures of rice are heaped round the base of it. It is called *karagam*, i.e., the pot, and is carefully prepared at the chief local shrine of Kurumbaiamma about a mile outside the village, and during the festival is treated exactly like the goddess. It is taken round in procession on the head of a pujari to the sound of tom-toms and pipes; offerings of fruit and flowers are made to it; a lamb is sacrificed before it, and it is worshipped with the orthodox prostrations."||

We have noted, incidentally, that in ancient Egypt the god himself may be represented by sand. There are other examples of this. According to Mrs. Sinclair Stevenson, in India the Hindu goddess Gauri is represented sometimes by five small

* Westermarck: "Marriage Ceremonies in Morocco," 1914, p. 94.

† "The Rites of the Twice-born," 1920, pp. 286f.

‡ Baldwin Spencer and F. J. Gillen: "The Northern Tribes of Central Australia," 1904, pp. 230ff.

§ Jesses Walter Fewkes: "Notes on Tusayan Snake and Flute Ceremonies," in the *Report of the Bureau of American Ethnology*, 1897-98, Part 2, p. 989. The American Reports give a vast amount of information concerning ceremonial sand-drawings among the North American Indians.

|| "The Village Gods of South India," 1921, pp. 37f.

heaps of sand on each of which five small pebbles are placed.* Again, every Sunday in the month of Jyestha, when young girls worship Parvati, the wife of Siva, "they go down to the river bank, make heaps of sand there, and worship them, calling them Parvati."† A village deity in South India is represented in the same way. Bishop Henry Whitehead says he often saw on the seashore of Madras "a conical heap of sand, about three inches high, standing on a small platform of sand, with camphor and incense in a small earthenware vessel or in a heap of old netting. The conical heap of sand represents the goddess Kanniamma, the gramadevata of the fishing village."‡

Sand is used in much the same way as salt to protect against evil or the evil eye, and to cure illnesses. S. Seligmann says that in northern lands a whining child is taken to a church at midnight, and a little bag of sand taken from a heap before the altar is tied round its neck.§ In Naples a little bag of sand is carried as a protection against the *Jettatura*.|| In Cape Town, where Muslim magic and so-called Malay doctors are believed in, "amber beads, dried dates, flowers, Zem Zem water, and sand or earth from Mohammed's grave are all used for good luck."¶ We are told of an early Christian Father, Petarpemotis, who worked miracles, that "when the people mixed the sand which he had blessed with the sterile soil of their land, crops grew straightway which were larger and more abundant than those of any other part of Egypt."**

Let us now take a few examples of the use of salt. In modern Egypt when a child is seven days old, the midwife carries it about the house, scattering, according to one writer, salt, sweetmeats, and small coins; according to another, wheat, barley, peas, and salt. On the eve of the festival which follows the month of Ramadan, to protect their houses against evil, some of the women in Egypt sprinkle salt upon the floors of the apartments, saying: "In the name of God, the compassionate, the merciful."†† In Morocco people protect themselves against evil by strewing salt on the floor of their houses or tents on the twenty-seventh night of Ramadan.‡‡ To protect sacrificial animals, salt is thrown on the spot where they are to be slaughtered, or pushed into their mouths just before they are killed, or put into the gaping wound and thrown on the blood on the ground.§§ To protect animals against the evil eye a small piece of salt tied in a black rag with a red string may be used.|||| Again, we are told that the farmer hangs a piece of salt on the animal with which he is ploughing.¶¶

There are other, more ordinary uses of sand. It is used in fortune-telling and divination. In the Siwah Oasis fortunes are told by means of rows of prints made in the sand with the finger tips. Or a square is made on the sand, letters are written on it in a circle, and the man whose fortune is to be told is ordered to put his finger on one of the letters. This is the well-known form of divination called by the Arabs *darb-er-raml*.*** The Malagasy have a method of divination which S. P. Oliver thinks

* "The Rites of the Twice-Born," 1920, pp. 51f.

† *Ibid.*, p. 301.

‡ *Op. cit.*, p. 28.

§ "Der Böse Blick und Verwandtes," 1910, I, p. 281.

|| Seligmann, *op. cit.*, II, p. 39.

¶ S. M. Zwemer: "The Influence of Animism in Islam," 1920, p. 174.

** E. A. Wallis Budge: "Osiris and the Egyptian Resurrection," vol. II, p. 183.

†† "Harvard African Studies," I, 1917, p. 3.

‡‡ Westermarck: "The Belief in Spirits in Morocco," 1920, p. 145.

§§ Westermarck: "The Moorish Conception of Holiness" (Baraka), 1916, p. 135.

|||| Westermarck: "The Belief in Spirits in Morocco," 1920, p. 51.

¶¶ Westermarck: "Ceremonies and Beliefs connected with Agriculture, etc.," 1913, p. 16.

*** "Harvard African Studies," I, 1917, p. 14.

peculiar. "It is neither based on astronomy, necromancy, nor magic; but its nature is oracular, and calculated from a fixed process of the permutations and combinations of certain straws, beans, or sand, placed in particular lines and positions."*

Sand is used also as a substitute for water. Muhammad is reported to have said: "If ye be sick, or on a journey, or have come from the unclean place, or have touched a woman, and ye find not water, then rub pure sand, and bathe your face and your hands with it."† In the desert advantage is often taken of this concession, sand being used for the prescribed ablutions.‡

Let us return to Knutsford. Sand, like salt, is a life-preserving, fertility-promoting, luck-bringing substance. As life-preservers, both sand and salt were used in ancient Egypt in mummification. A magical potency came to be ascribed to them, possibly in quite primitive times. If they could preserve the body of the dead, they could also create life or re-create life in the body of the living. The verses I have quoted referring to sanding in Knutsford preserve a hint that sand was a fertility substance. This hint, in the light of my investigation of various uses of sand, looks like a certainty. How and whence sanding arose in Knutsford still remains a problem.

MAURICE A. CANNEY.

Prehistory: Ice Age.

Moir.

The Ice Age. By J. Reid Moir.

19

Mr. Burkitt (MAN, 1926, 5) states: "The cold climate of the Chillesford beds, followed by the slight warming at the base of the Cromer Forest series, would seem hardly sufficient to postulate a glaciation and an inter-glacial period." As I am responsible for this "postulate," and as Mr. Burkitt's remarks are misleading, I would like to comment briefly upon them. The facts of the matter are as follows: (a) at the base of the Red, Norwich, and Weybourne Crag there are present large quantities of heavily striated flints and numerous examples of ice-borne, foreign rocks; and (b) an examination of the molluscan remains in these Crag demonstrates that the period in which they were deposited was one of ever-increasing cold, culminating in entirely boreal conditions. The Crag epoch of which the Chillesford episode forms but a part, was evidently of considerable length—and, essentially, cold. Immediately above the boreal Crag deposits, in Norfolk, occurs the Cromer Forest Bed, containing, throughout its thickness, very numerous remains of animals that, with two rare exceptions, are such as can live only in a warm climate. It is clear that the Forest Bed deposits are representative of a prolonged period, during which great changes in the surface of the earth took place, and that this period was essentially warm. In my judgment the only rational explanation of this matter is that the Crag deposits represent a glaciation, and the Cromer Forest Bed an inter-glacial epoch. I fail, also, to understand what Mr. Burkitt means in stating that "We should have to consider the *lower part* (my italics) of the chalky boulder clay as Riss." There are two Boulder clays in Suffolk, the older (which is probably equivalent in age to the Glacial Till and Contorted Drift of Cromer) being known as the Kimmeridgian Chalky Boulder Clay. While the more recent deposit, separated, apparently, from the lower by a well-marked inter-glacial epoch, is called the Upper Chalky Boulder Clay. To which of these separate and distinct beds is Mr. Burkitt referring when he speaks of the "lower part of the chalky boulder clay"? §

* "The Hovas," pp. 16f.

† "Qur'an," iv, 46; cp. v, p.

‡ J. C. Ewald Falls: "Three Years in the Libyan Desert." 1913, p. 292.

§ I presume that the term "under-typography," used by Mr. Burkitt three times in his note, should be "under-topography"?

I notice that Messrs. Peake and Fleure (MAN, 1926, 3) claim that no evidence of the *Günz* glaciation is present in this country. I would suggest, however, that the cold period of the East Anglian Crags represents this glacial epoch; that the Cromer Tills, Contorted Drift, and the Kimmeridgian Chalky Boulder Clay equals *Mindel*; that the Upper Chalky Boulder Clay equals *Riss*, and that the *Würm* is represented in the eastern counties by hill-washes of considerable thickness, and by the Arctic flora found at Barnwell, near Cambridge. Further, I see no escape from the conclusion that the true Chelles horizon is to be located in the Cromer Forest Bed, and may thus be of *Günz-Mindel* inter-glacial age. All these views were, however, first published by me in the *Geological Magazine* (Vol. LVII, No. 671, May) as long ago as 1920, a fact which seems to be forgotten by recent writers upon this question.

J. REID MOIR.

Germany : Archæology.

Hauser.

La Micoque Industry : Remains of the Old Stone Age in Germany.

By Dr. O. Hauser.

20

During the last few months highly important discoveries of artifacts of palæolithic age have been made in various parts of Germany. Thus I have been

able with absolute certainty to identify such cultures of the Old Stone Age in Vogtland, Lausitz, Saxony as a whole, Thuringia and South Germany. All the artifacts are derived from strata indisputably dating from the third inter-glacial epoch, and undoubtedly belong to the evolutionary phase represented by La Micoque and Ehringsdorf. In Fig. 1 is shown a perfectly self-contained series of German palæoliths which is now for the first time available to illustrate vividly the culture of a diluvial archæological epoch. All forms of the industry of La Micoque are represented—the splendid pointed hand-axes (*Keildolche*), notched side-scrapers, keeled

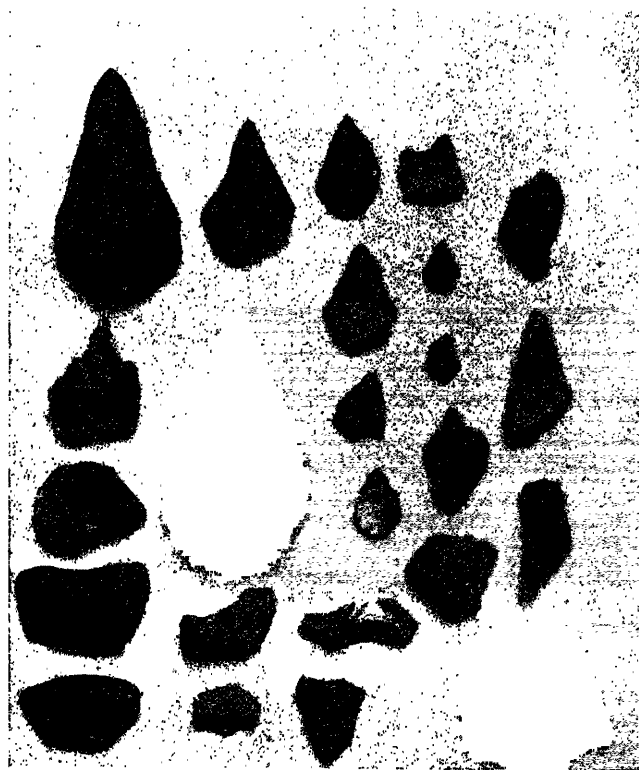


FIG. 1.—“LIFE SERIES” OF GERMAN PALÆOLITHS.

scrapers (*Kielschaber*), common side-scrapers, double side-scrapers of dimensions between 40 and 70 cm., and points such as appear in the classical Mousterian

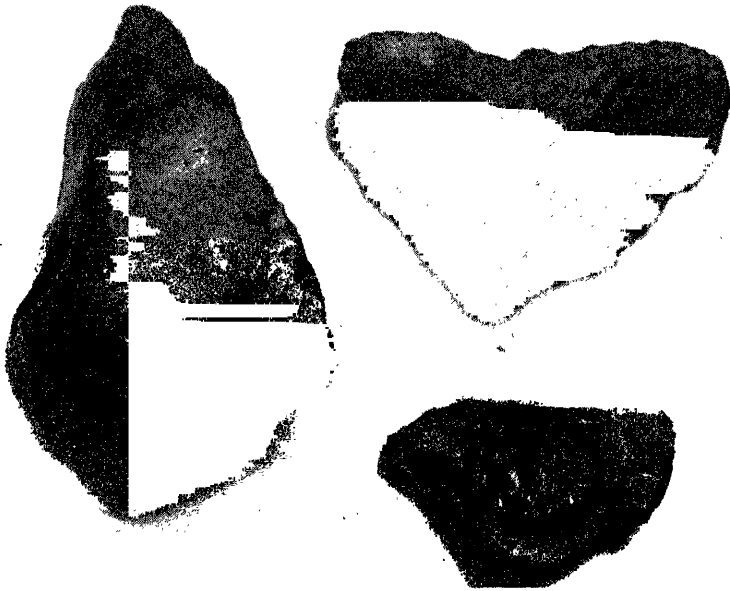


FIG. 2.—EXAMPLES OF LA MICOQUE INDUSTRY OF GERMANY.

culture and, as is well known, gradually vanish in the La Micoque phase to make room for more highly-developed instruments. O. HAUSER.

REVIEWS.

Europe: Archæology.

Xanthoudides.

The Vaulted Tombs of the Mesara: an Account of some early Cemeteries of southern Crete, by Stephanos Xanthoudides, translated by J. P. Droop, with a preface by Sir Arthur Evans. The University of Liverpool Press, 1924. xx + 142 pp. and LII plates. Price, £4 4s. 21

The University of Liverpool and Professor Droop deserve the thanks of all archæologists for publishing so sumptuously in English the reports of Dr. Xanthoudides' excavations in southern Crete. The results of his commendably careful and accurate observations of the ruined *tholoi* cast a flood of light on a whole host of questions of general prehistory. These huge corbelled tombs were, of course, collective sepulchres in use for many generations and containing thousands of corpses. Each vault in fact covered objects belonging to two or even three successive phases of the first five Minoan periods and sacrilegious hands had been laid upon many of the older valuables in making the later interments. Nevertheless enough remains to show that some of these *tholoi*, such as those round Koumasa, were actually built in the first epoch of Minoan civilisation (E.M.I.). In the light of this fact the Libyan, and especially Egyptian, affinities both of the sepulchral architecture and of the grave goods, have a peculiar value in elucidating the origins of the first civilisation in Europe. Sir Arthur Evans in his preface and the author in his last chapter have enumerated and annotated the Nilotic parallels to the plan of the tombs with their annexed structures and to the palettes, tweezers, seals, scarabs and amulets that they contain. At the same time relations with the Cyclades,

exemplified by numerous marble figurines, were peculiarly intimate on this southern shore of the great island.

But what particularly deserves the attention of English archæologists is the surprising parallelism between these tombs of the eastern Mediterranean and the megalithic sepulchres of the Atlantic coasts. In the light of Dr. Xanthoudides's thoroughly scientific study of their relics many of the problems presented by our own long-barrows and chambered cairns, which have often been examined in a far less satisfactory manner, assume a quite new aspect.

The corbelling of the Mesara vaults takes back to the third millennium the assumed prototypes for Uley and Los Millares. The portal of the burial chamber was composed of genuinely megalithic uprights and the huge monolithic lintel "humped on the back to relieve the central pressure" is not only a modest forerunner of the lintels of the famous Mycenæ tombs, but has precise parallels in Breton and Scottish sepulchres of the "stone age." As in the collective tombs of the West, the bones were often in confusion and in some cases had been blackened by fire. But these facts indicate neither secondary burial nor *scarnitura*, nor yet cremation. A few undisturbed skeletons show that the corpse was originally laid to rest in the contracted attitude, sometimes in a clay or wooden chest. The confusion of the skeletons was due to the disturbance created in making room for later interments, and the smoke stains resulted from huge fires kindled in the vaults perhaps to clear the foetid atmosphere of the charnel house before the deposition of fresh corpses. The disorder of the bones and the cinders observed in Western megaliths must be interpreted in the light of these observations.

The furniture of the tombs is no less instructive from this standpoint. For example, a whetstone perforated at the four corners looks extraordinarily like one of our own "bracers," but there is no doubt as to its actual use for sharpening metal implements. And to take one Central European parallel, the round discs of gold foil, presumably used for casing buttons, illustrate a type extremely common in bronze during the Middle Bronze Age. Incidentally a jadeite celt from Kalathiana and the tin in E.M. III daggers deserve mention in this context. A student of the problems of the Atlantic coasts can no more dispense with a thorough study of this fascinating work than can a professional Ægean archæologist. And, thanks to Professor Droop's graceful translation, such a study is a pleasure.

V. G. C.

America: Physical Anthropology.

Hrdlička.

The Old Americans, by Aleš Hrdlička. Baltimore: Williams-Wilkin Company. 1925. Pp. 438, 38 plates.

22

Of the many contributions which Dr. Aleš Hrdlička has made to the literature of Physical Anthropology in the past thirty years, none is of higher quality and of more immediate value than the long and painstaking research he has now published in book form under the engaging title "*The Old Americans*." The "*Old Americans*" are men and women now living in the United States who can claim that their grandparents, like themselves, were born under the "Stars and Stripes." Nearly one thousand such individuals have been minutely examined, measured and recorded. Dr. Hrdlička, in the treatment of his data, has divided them into three groups—(1) a laboratory group, which includes, for the greater part, men and women belonging to the northern states of the Atlantic seaboard; (2) an "engineer group"—measured by Prof. R. Bennett Bean—belonging to southern states of the Atlantic seaboard; (3) Appalachian mountaineers—representatives of a British stock long settled in the uplands of the State of Tennessee. Although drawn from sources

which are widely separated, these three groups, when subjected to Dr. Hrdlička's minute process of analysis, emerge as members of the same stock—the Old American. This is just the stock that all the world wants to know something definite about, something that can be relied on, and something which will serve as a basis for further knowledge. It is true that the Old American stock has been brought into the limelight of late by a number of popular books which tell us much of what their authors think, but nothing of what they have observed. Dr. Hrdlička and his collaborators, on the other hand, tell us much of what they have observed and help us by their thinking to understand what their observations signify.

Dr. Hrdlička regards the Old American as the direct progeny of a stock which drew at least seven-eighths of its blood from Britain. The question he set out to answer was: have these Old Americans developed into a new type during the centuries they have lived in another continent, or are they, in their physical type, still essentially British? It may be said at once that Dr. Hrdlička has found nothing to support a widely held belief that the "Old American" is assuming the physiognomy of the American Indian. After comparing the physical characters of the Old Americans to such data as he could find relating to the peoples of Europe, he comes to the conclusion that the Old Americans resemble the natives of the British Isles much more than those of any other country in Europe, and, of the various constituent elements of the British people, they are most like to the English. Although the Old Americans are essentially British in body, yet there is distinct evidence that a new physical type—an American-British type—is being evolved. Pigmentation of hair and skin tends to become more uniform, less scattered in its grades, than among the British people; the men tend to be taller; the mean stature for the men measured by Dr. Hrdlička is 174.2 ctm. (5 feet 8½ inches) and for the women 161.8 ctm. (5 feet 3.7 inches), which are equal to measurements of British professional classes. The men are heavier and their chests are bigger than with us; the feet of the men, and more particularly of the women, are longer and narrower. The cephalic index (78.2) comes very close to our British mean, but the absolute diameters of the head are distinctly larger than in us and we must infer that the Old American brain is bigger than the British. We should not have been surprised if Dr. Hrdlička's observations had demonstrated the evolution of an "Old-American" face, but in its width and length, as in the measurements of nose, chin and bigonial width, the American and British faces agree very closely.

Dr. Hrdlička has been handicapped by the lack of measurements relating to the peoples of Europe—particularly of the people of Britain. We have never observed, as he has done, the onset of baldness and of graying of the hair. It is to be hoped that we may be able to supply the comparative data needed to complete Dr. Hrdlička's investigations; our Universities have collected some of the measurements needed and it should not be difficult, at such centres as Cambridge, Oxford, Dublin, Edinburgh and Aberdeen, to complete the needed series of observations. Particularly interesting would it be to compare the Old Americans with "Old" Australians and "Old" New Zealanders.

As to the American type of the future, it will be well to reproduce Dr. Hrdlička's own words:—"From the remainder, doubtless a large majority, particularly in the cities, there is forming and will result a conglomerate which through ever-increasing intermixture may doubtless in the course of a few generations be expected to approach a newer blend—the American type of the not far distant future. This type, we may surmise from all available data, will not be far from the Old-American type of the present, and yet will be somewhat different, particularly in the physiognomy and in behaviour. The Neo-American type will in all probability be, in the average, tall, more sanguine, and perhaps less spare than

" the Old." From which it will be learned that Dr. Hrdlička holds out little hope of America saving its old type. That type is being drawn into the most colossal experiment in race-building the world has ever seen; perhaps the greatest it will ever see.

ARTHUR KEITH.

CORRESPONDENCE.

Sudan: Ethnology.

Seligman.

To the Editor of MAN.

Tribes of the Southern Sudan.

SIR,—I must thank Lord Raglan for his courteous criticism of my Presidential address. He is one of the few men who has studied the question on the spot, and his opinion must needs command respect. Nor is the object of this letter to confute it with regard to the order of the Dinka and Shilluk waves. 23

My reason for writing is to express my dissent from the view that there was a single "homeland" for the Dinka-Shilluk group (which includes more than these tribes) and the Lotuko-Turkana-Masai group. I do not believe that these two groups shared a single cradleland, and when on page 26 of my address I wrote of a Nilotic cradleland I certainly did not intend to include the Lotuko, Turkana, Masai, etc.

Yours faithfully

C. G. SELIGMAN.

Toot Baldon, Oxford,
1st January, 1926.

ANTHROPOLOGICAL NOTES.

Exhibit of a Set of Models Illustrating the Study of Flint Flaking.—An exhibit of models illustrative of flint flaking, made by Mr. S. Hazzledine Warren, F.G.S., will be held in the Lecture Room of the Royal Anthropological Institute at 52, Upper Bedford Place throughout the month of February, during the hours at which the Institute is open. Fellows and others who may be interested in the subject are cordially invited to visit this exhibition and examine the models. They number over 60, and are planned on similar lines to the models used in the study of comparative anatomy and other subjects. That is to say, they are painted in conventional colours to indicate the comparisons of homologous parts as developed under various human, mechanical, and natural processes of flaking. 24

It is intended to make a limited number of duplicate sets of these models, as a first attempt towards a standard set for comparison.

So far as good natural flakings are concerned, so few have yet been collected that for the time being Museums generally do not, and cannot, possess adequate specimens. Although plaster casts are inferior to originals, it is hoped that they may prove better than none.

The Rivers Memorial Medal.—The Rivers Memorial Medal for Anthropological work in the field for 1925 has been awarded by the Council to Professor C. G. Seligman, M.D., F.R.S., for work in New Guinea, Ceylon and the Sudan. The Medal was presented to Professor Seligman by Dr. A. C. Haddon at the Anniversary Meeting of the Institute on 26th January.



FIG. 1.—PROTO-MESOPOTAMIAN PAINTED WARE FROM TELL ZEIDÂN.

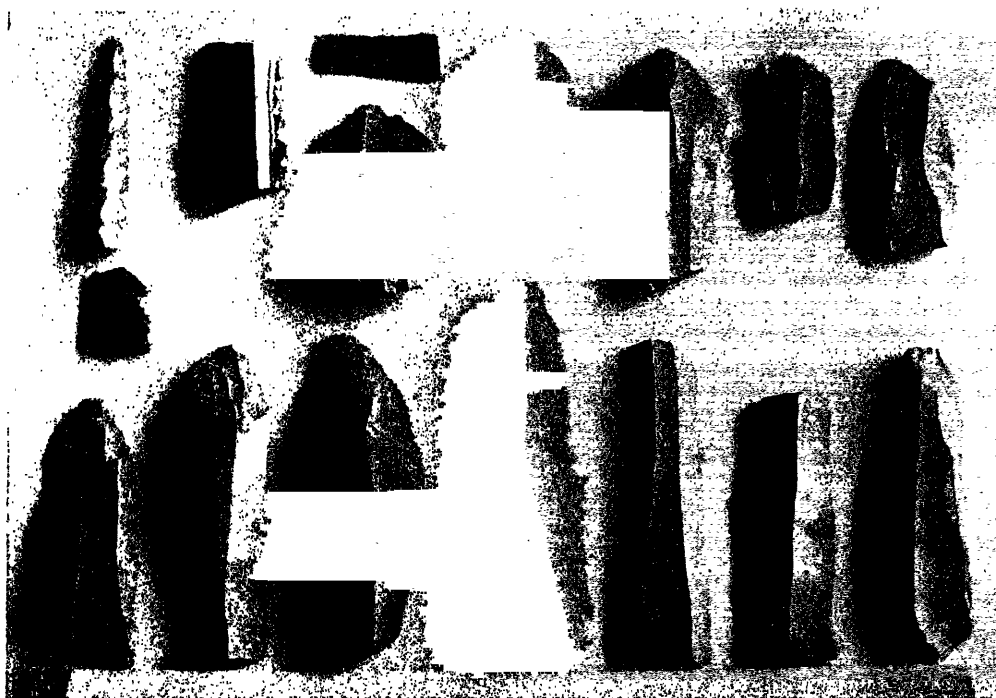


FIG. 2.—FLINT AND OBSIDIAN ARTIFACTS FROM TELL ZEIDÂN.

PROTO-MESOPOTAMIAN PAINTED WARE FROM THE BALĪKH VALLEY.

ORIGINAL ARTICLES.

With Plate C.

Mesopotamia: Archæology.

Albright.

Proto-Mesopotamian Painted Ware from the Balikh Valley. By **25**
W. F. Albright. With Plate C.

In the autumn of 1925 Professor R. P. Dougherty and I devoted some weeks to a surface examination of the mounds of the Middle Euphrates region, with unexpectedly interesting results for the comparative ceramics of the æneolithic and Early Copper (= Early Bronze) ages. Our most valuable data for the æneolithic period came from Tell Zeidân, a mound about 500 metres in length, which stretches along the eastern bank of the lower Balikh river. This mound is covered with innumerable potsherds and flint artifacts, all remarkably homogeneous in character, nor were any traces of later occupation found in the vicinity. The pottery, some sherds of which are illustrated in the accompanying figure, is nearly all creamy white or light buff in colour, with geometric painting in black (often much faded) or reddish brown. When the ware is white or a very light buff, the paint is usually applied without a slip, but most of the light buff ware has a creamy white slip. The only polychrome sherd found on the whole tell is marked *a* in Pl. C, Fig. 1; the bands are alternately dull black and brownish red on a white slip over a light buff paste. The same ware is also found at Tell es-Semen on the Balikh. The artifacts found with the pottery consist of knives, arrow- and lance-heads in flint and obsidian, as shown in the figure.

During our travels in Mesopotamia we also studied the similar pottery of the æneolithic age found at Abu Shahrein, Tell el-'Obeid and elsewhere in Babylonia, as well as at several sites in the East Tigris country and Assyria, from all of which we made collections which supplement the sherds from Tell el-'Obeid and Kerkûk now in the Baghdâd Museum. Thanks to the kindness of Miss Bell we were able to compare our pottery with the material from these places in the Museum. While this is not the place to go into details, I shall briefly sketch the principal results of this study, basing it upon the admirable works of Pottier, Thompson, Hall, and now especially of Frankfort, in his brilliant monograph entitled *Studies in Early Pottery of the Near East*, I, London, 1924.

There is a similarity which almost amounts to identity in the pottery from north-western Mesopotamia, Babylonia and southern Susiana (Bender Bushîr) in the pre-monumental age. This ware is all highly developed from the standpoint of technique, although the processes employed were simple. Thus the vessels from Tell Zeidân and Tell el-'Obeid are not made on the tournette, but are "hand-turned," to use Mr. Woolley's happy expression (*see* Frankfort, *op. cit.*, p. 8). The painted ornament is either geometric imitation of basketwork or is stylised naturalism (with a magical base), as in the case of a water-fowl from Tell Zeidân, where the marsh is indicated by a network of intersecting lines (= reed thicket), and a group of wavy lines (= water). The shapes differ somewhat. Thus, at Tell Zeidân, we find almost exclusively tumblers and bowls with straight sides and simple ring bases, as at Susa I. In southern Babylonia these vases are common, and resemble the corresponding types from Tell Zeidân almost perfectly; but there is also a series of thicker walled, carinated rim forms which are absent from Tell Zeidân, and point the way to the common carinated types of the Early Copper (third millennium). The wares from the region of Kerkûk (Khazneh Tepe, etc.) and Tepe Mussiân, as well as those from Assyria proper, belong to an intermediate category, with numerous points of contact. Characteristic

of Assyrian æneolithic ware is pottery with lustrous paint, which at first sight reminds one of Ægean ware, but is really quite distinct, though perhaps ultimately related.

Of great importance for the relative dating is the relation between the early painted ware and the incised ware which finally took its place completely in the third millennium. At Tell Zeidân we found no incised ware, except a few fragments of bowls with deep incised lines crossing one another *inside* them. This same curious type of interior incision appears also at Abu Shahrein, Tell el-'Obeid and Kerkûk, but disappears with the painted ware. At Khazneh Tepe near Kerkûk, as well as in Assyria proper, painted and incised ware appear together, but the incised ware tends even here to appear at higher points on the mounds than the former. At Khazneh Tepe, where the difference in levels is not so clear, there is a marked archaism about the incised ware, distinguishing it from the typical pottery of the Early Copper. The Early Copper incised ware of all parts of Mesopotamia is practically identical with the incised ware of the G and F strata at Assur, which must be dated with Andrae (*Die archaischen Ishtar-Tempel in Assur*) cir. 3000-2500 B.C. At Assur, Andrae found painted pottery of our æneolithic type among the foundations of stratum H, though, to judge by slightly conflicting statements in his treatment (which do not reduce the value of this superb piece of work) he was not clear whether the painted ware was still employed by the H-people or not. Since G was pre-Sargonic (*i.e.*, older than the twenty-eighth century), H must be dated toward the end of the fourth millennium, and the disuse of painted ware in Assyria can hardly be dated after 3000 B.C., at the latest. If we turn to southern Babylonia we secure more precise results. The use of painted ware was confined to the earliest stratum of Eridu (Abu Shahrein); the upper stratum, which is Sumerian of the first half of the third millennium, replaces it entirely by Early Copper incised pottery. At Tell el-'Obeid we find that the painted pottery technique either had gone out of use or was falling into disuse by the time of A-anni-padda of the First Dynasty of Ur, who cannot be placed later than about 3100 B.C., if we date Ur-Nina of Lagash in the thirtieth century. At Kish in northern Babylonia we find typical Early Copper incised in the *débris* of the early colonnaded palace excavated by Mackay, suggesting that the painted ware had already gone out of style here. We shall probably not be far off if we date Susa I in the first half of the fourth millennium, Tell Zeidân roughly about the middle of this millennium, the early occupations of Eridu and Tell el-'Obeid slightly later, and the painted ware of the Kerkûk region in the second half of the fourth millennium.

In the title of this communication I have used the colourless designation "Proto-Mesopotamian." "Proto-Sumerian" is just as correct, in my opinion, since the Mesopotamian painted ware was employed by the race which founded the cities of the region where Sumerian place-names are found from the highest antiquity. It is, however, much better to avoid a racial name for pottery, which is a cultural phenomenon, not a racial trait.

W. F. ALBRIGHT.

Britain: Archæology.

Stone.

Stonehenge—the Supposed Blue Stone Trilithon. By E. Herbert
Stone, F.S.A.

26

Stone No. 150, now lying prostrate in the eastern portion of the blue stone circle, has on its present upper surface two cup-shaped hollows. The stone lies with a sideways tilt and is half buried in the ground as shown in the accompanying photograph. The position of the stone in relation to the other stones of the structure is shown in the plan, Fig. 1 (p. 44).

At what period in the history of Stonehenge these cup-shaped hollows were formed in this fallen stone—and what purpose these hollows were intended to serve—must remain a matter of conjecture.

From a supposed analogy to the mortise sockets in the lintels of the great sarsen stones some persons have assumed that the hollows in blue stone No. 150 indicate that it had been the lintel of a miniature trilithon forming a part of the original design of Stonehenge.

With this idea to start with it has been argued: "If there had been one such dwarf trilithon, why not also let us have another to correspond—or, indeed, a whole circle of these structures?" To support such theories it was of course necessary to find places for these supposed blue stone trilithons. Some of the suggestions are as follows:—



THE SUPPOSED BLUE STONE TRILITHON.

(a) That the small trilithon occupied a place on the blue stone circle, stone No. 32 having been one of its uprights.

(b) That the small trilithon occupied a central position somewhere on the Axis.

(c) That there were two small trilithons, one at the end of each limb of the blue stone horseshoe, stone No. 61 having been one of the uprights.

(d) That there were two small trilithons, one on each side of the Axis, placed so as to form in plan a complete ellipse in continuation of the great sarsen trilithons.

(e) That there was a complete circle of small blue stone trilithons.

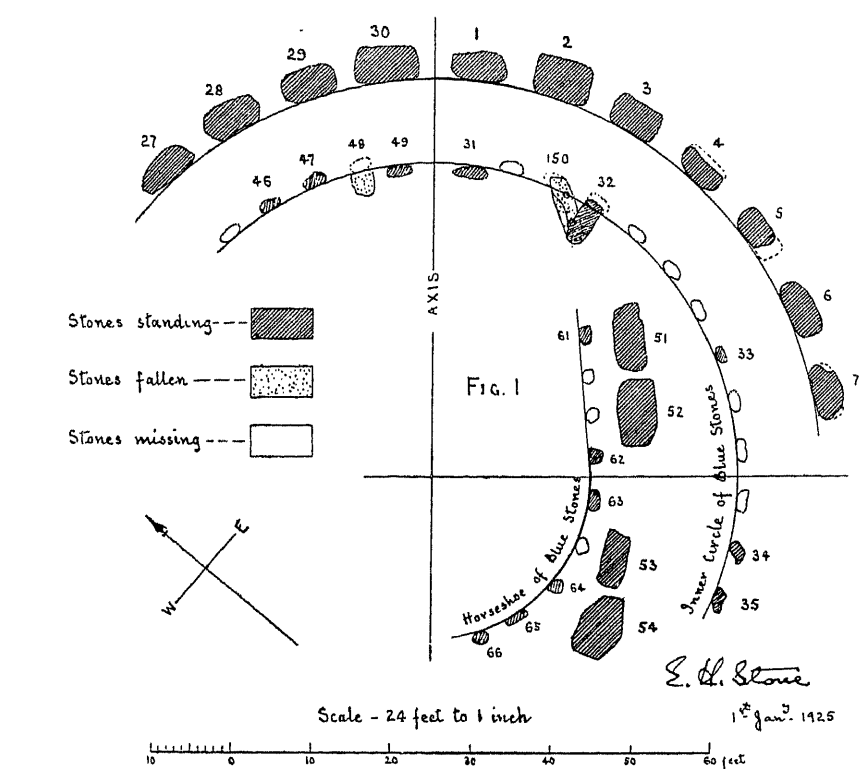
NOTE.—It will be observed that each of these theories involves the assumption that stone No. 150 having fallen was afterwards removed and deposited in the place where we now find it—and that, later on, stone No. 32 fell over it.

All this, of course, is mere conjecture, and, if plotted on a large scale plan, it will be evident that these theories are all more or less unsatisfactory. In any case it will be realised that a pigmy blue stone trilithon, standing alone, would be a somewhat ridiculous object and quite out of harmony with the general architecture of Stonehenge.

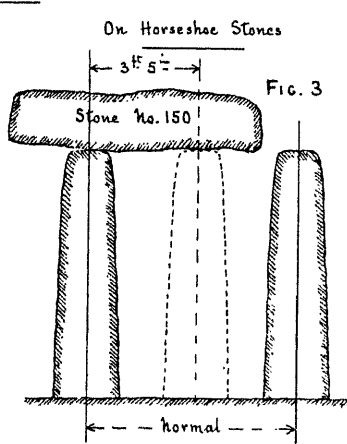
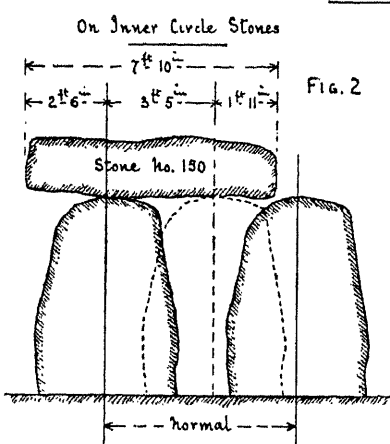
The position of the cup-shaped hollows in stone No. 150 is such that it may be regarded as most improbable that they could have been mortise sockets for a trilithon, and the shape of the stone is not at all suitable for a lintel. The two hollows are unsymmetrically placed askew with each other, and only 3 feet 5 inches apart from the centres. The one towards the north is 2 feet 6 inches from that end of the stone, and the other is 1 foot 11 inches from the further end.

In Fig. 2 is shown stone No. 150 as it would appear supported on two of the largest stones of the blue stone circle. In this case the inner sides of the uprights would overlap. With a pair of smaller stones of the blue stone circle the trilithon would be quite absurdly small. In Fig. 3 is shown stone No. 150 as it would appear supported on two stones of the blue stone horseshoe.

STONEHENGE
THE SUPPOSED BLUE STONE TRILITHON



The figures below shew the conditions if Stone no. 150 were mounted as a lintel on a pair of blue stones - The hollows in Stone no 150 are 3^{ft} 5" apart (centres) -



Scale - 6 feet to 1 inch

On this matter Flinders Petrie, writing in 1880, remarks :—

“ It [stone 150] will not fit any of the existing blue stones, as the mortises are too close together for any now standing; and it cannot be intended for one of a set of continuous lintels, as its ends beyond the holes are too long.”—(*Stonehenge*, p. 17.)

It is further to be observed that among the blue stones now remaining on the site there is no stone that has any sign of a tenon on its top, or which bears any indication that it may have been an upright for a trilithon. Moreover the stones are very irregular in size, shape and height—there are no two alike, and in the blue stone circle the stones are merely shapeless boulders. There is no other blue stone which has cup-shaped hollows like those on stone No. 150.

On reference to the plan (*Fig. 1*) it will be seen that the eastern ends of stones Nos. 150 and 32, as they now lie, are just about on the sites which would be occupied by two stones corresponding in position with stones Nos. 46 and 47 on the other side of the Axis (*see dotted outlines*). If we neglect the trilithon theory it would be evident that stones Nos. 150 and 32 are stones of the blue stone circle which occupied these sites and have fallen inwards.

If, on the other hand, we were to accept the trilithon theory *we should thereby be bound to assume that the stone which had occupied the site had at some time been removed, and that stone No. 150 had afterwards been carried for some distance from the place where it fell and placed just in the position which had previously been occupied by the original stone.*

It is obvious that if stone No. 150 had been the lintel of a trilithon it could not have fallen in the position it now occupies.

The opinions of various authorities on this matter are given in detail by the writer in his work on *Stonehenge* (pp. 14–18). The conclusion there arrived at is that stone No. 150 is not the lintel of a trilithon, and that the cup-shaped hollows now to be seen therein have nothing to do with the original design of the structure, but were probably the work of pre-historic squatters on the site when Stonehenge was already in a partly ruinous condition—may be a thousand years or more after the date of its construction.

If this stone had been dug up in the course of excavation on the site of a pre-historic village it would doubtless have been agreed, without question, that the cup-shaped hollows had been formed as mortars for grinding grain.

In this connection Edward T. Stevens, the eminent Wiltshire archæologist, writing in 1876, remarks as follows :—

“ The two cavities in the prostrate foreign stone are too far from the ends of this particular stone and too close together to justify one comparing it with the imposts of the outer circle or outer horse-shoe. No trace remains of either of the syenitic uprights upon which it rested. This stone, however, is quite as likely to have served for an altar as for an impost, and the cavities may have been intended to receive libations or offerings of some kind.”

Stevens then goes on to describe similar stones found in Sweden, known as “elf-stones,” which are still held in superstitious veneration and receive offerings to ward off sickness. He adds :—

“ I venture to suggest, therefore, that some further attention be given to this subject, before we jump to the conclusion that this foreign block of stone was an impost.”—(*Jottings on the Stonehenge Excursion*, August, 1876, pp. 133–138.)

E. HERBERT STONE.

Anthropology: Physical.

Pearson.

On the Reconstruction of Cranial Capacity from External Measurements. *By Professor Karl Pearson, F.R.S.***27**

In a paper with the above title in MAN for October, 1925, Mr. Dudley Buxton refers to the work on reconstruction of cranial capacity by Dr. Lee and myself, and suggests that a certain formula given by us is inapplicable to prehistoric crania, and asks that the authors of it should give some test of its applicability. The authors of it seem to me to have given very definite warning of the danger of applying reconstruction formulæ to races widely divergent from those upon which they were based. The memoirs issued from the Biometric Laboratory on this subject are the following:—

(i) Lee and Pearson: *Philosophical Transactions*. Vol. 196, A., pp. 225-264.

(ii) Lewenz and Pearson: *Biometrika*, Vol. III, pp. 366-397.

(iii) Leon Isserlis: *Biometrika*, Vol. X, pp. 188-193.

(iv) Beatrix Hooke: *Biometrika*, Vol. XVIII. This paper is now at press, and I have delayed replying to Mr. Dudley Buxton until I could use the new results worked out by Miss Hooke.

Before referring to the formulæ and conclusions reached in the above papers, I desire to point out that in both (i) and (ii) the distinction between two types of formulæ has been emphasised, namely: (a) the *intra-racial* formula, which gives the best value for an individual within a race, and (b) the *inter-racial* formula, which endeavours to give the best value of the *mean* capacity of a race from the *mean* values of the characters in that race.

On this distinction Lewenz and Pearson wrote in 1904:

"In the light of modern scientific enquiry we demand that the craniologist shall distinguish between what holds for a local race of men, and what may be applied to mankind as a whole. We have elsewhere shown by actual measurements that inter-racial and intra-racial correlations are not the same, and consequently the reconstruction formula for the individual within a given race is not the same as the formula for reconstructing the mean of a given race." *Loc. cit.*, p. 397.

Now Formula (10) for males, namely

$$\text{Capacity} = \cdot 000365 \text{ L.B.OH} + 359 \cdot 34,$$

which for some reason, unknown to me, Mr. Dudley Buxton says we lay most stress on, and which is clearly the best, he tells us, for male crania, is an inter-racial formula and ought only to be used for finding *racial means*. It is based on the racial means of some ten different races and should never be used to determine an individual capacity.* Yet Mr. Dudley Buxton writes that it differs widely from the *intra-racial* formula he has found for Eskimos and that it gives in all the cases he has tried very unsatisfactory results for Eskimo skulls. This was to be expected. If we apply it to the mean values which Morant has determined from Fürst and Hansen's values for Eskimo crania, namely: $L = 188 \cdot 4$, $B = 134 \cdot 4$, $OH = 120 \cdot 4$ we find our inter-racial formula gives for the capacity of the Eskimo race $1472 \cdot 1$.

* Sir Arthur Keith, and even such an expert biometrician as Miss M. Tildesley, seem equally to overlook the difference between an intra-racial and an inter-racial formula. Pearson and Lee (*loc. cit.*, p. 238) state their *First Fundamental Problem* to be: *The Reconstruction of the Individual from the known Formula for his own Race*, and (p. 242), *Second Fundamental Problem on the Determination of the MEAN skull capacity of any local race of man from the regression formulæ for a second race*. Sir Arthur Keith and Miss Tildesley have selected a formula given under this *Second Fundamental Problem* to determine the capacity of an *individual* skull, i.e., the Chancelade. Professor Sollas did better, he took an intra-racial formula, but he chose a very bad one—one, namely, for a very brachycephalic race showing few, if any, of the characters peculiar to the Chancelade type.

Now let us consider this result from the standpoint of four other inter-racial formulæ provided by Lewenz and Pearson (*loc. cit.*, pp. 371, 386 and 388). These are not based on the three lengths product, but on the three arcs product, U, S and Q.

The first, which the authors term G.F., is based upon the mean value of four intra-racial formulæ; it is:

$$\text{Probable Capacity} = 244.6 + .01977 \text{ U.S.Q.}$$

For the Fürst and Hansen Eskimo:

$$S = 378.4, Q = 313.5, U = 523.8,$$

and we have $C = 1473.1$.

The second is a more truly inter-racial formula, being based on the *mean* values of the characters C, U, S and Q in *ten* different races, so wide a part as English, Aino, Malay and Negro. We have

$$C = 316.2 + .01852 \text{ U.S.Q.}$$

This gives for the Eskimos:

$$C = 1467.0.$$

But, by plotting the capacity measured to the arcual product for 19 races, Lewenz and Pearson (*loc. cit.*, p. 387) observed that a straight line does not adequately express the relationship; it is more closely given by a curved line. The curves fitted were (a) a parabola, (b) a logarithmic curve leading to the third and fourth formulæ:—

$$C = -15319.6 + .531,942 P - 4.2075 P^2/106,$$

$$C = 704.04 + 203.05 \log (P - 55682),$$

where P = the arcual product.

These two formulæ give for the Eskimos $C = 1488.5$ and $C = 1477.6$ respectively.

It will be seen that four independent formulæ based upon arcs give the capacity of the Eskimo skull as insensibly different from 1475.4, while the Pearson-Lee diametral formula gives 1472.1 in essential agreement. Mr. Dudley Buxton will, I am afraid, say that this is far less than the mean capacity determined from the Fürst and Hansen data as $C = 1527.7$. So it is, and still less than the value determined by Hrdlička for his Eskimos, namely, 1558.8. But it is in exact accordance with the mean capacity 1472.2 of 109 male Eskimo crania measured by Flower, Duckworth and Bessels. This fact has got to be explained. The diameters and arcs as measured by (i) Fürst and Hansen, (ii) Hrdlička, and (iii) Flower, Duckworth, Bessels, have closely accordant means, so close that we feel sure they are measuring the same race, but these three series give for the capacity widely divergent means. One can, therefore, be fairly confident that the capacities were not measured in the same manner.

The statement made by Fürst and Hansen as to measurement of capacity occurs on p. 141 of their great work. It runs:

“The cranial capacity has been measured with smooth millet seeds,
“ which run smoothly and pack better than others and fill up all the
“ corners of the cavity of the cranium. The measuring instrument was
“ a graduated glass cylinder.”

There is no statement as to the manner in which the most tight packing was reached in the case of either cranium or measuring glass. There is no reference to *Cranes étalons* as a means of control. It was the wide personal equation involved in tight packing in the graduated glass cylinder, which led to the weighing process in use in the Biometric Laboratory for avoiding the second tight packing*. It seems to me that the divergence as given by our inter-racial formulæ, which all

* See *Biometrika*, Vol. III, pp. 203-5.

agree among themselves, and Fürst and Hansen's results is due to a fundamental difference in measuring the capacity in the observations on which the formulæ were based and for the material of the Swedish investigators.

I do not know how Hrdlička measured his capacity, but his length and breadth are somewhat greater than for the two other series, while his total height exceeds that of Fürst and Hansen. In his paper on the Smith Sound Eskimo Hrdlička* says that he has used a variation of Flower's method, but the reference he gives for it is not accessible to me. The only inter-racial formula by which I can test Hrdlička's Eskimo capacity is that from Lee and Pearson's *Phil. Trans.* paper (*loc. cit.*, p. 247, Formula (12)), namely:—

$$C = 524.6 + .000,266 L \times B \times H',$$

which is based upon eleven races. This gives for Hrdlička's data: $C = 1485.3$, a value, as it ought to be, slightly above the values we have deduced for Fürst and Hansen's data, but much below the mean 1558.8 of Hrdlička series. It seems to me, therefore, that capacities as found in Germany and in the Biometric Laboratory are not comparable with those reached in Sweden and America.

If this be so the unique divergence in capacity of different observers is explained.

I now turn to the second point, having emphasised the fact that inter-racial formulæ must not be applied to determine individual capacity. Such a formula as that cited by Mr. Dudley Buxton must not be applied for intra-racial capacities.

The Biometric Laboratory workers have again and again insisted that regression formulæ vary from race to race.† Now we have the following intra-racial formulæ for capacity, where we confine our attention to *product* formulæ and write

$P = \text{arcual product} = (U \times S \times Q).$ ‡

$P_1 = \text{diametrical product} = (L \times B \times H')$, where $H' = \text{vertical height above basion}$.

$P_2 = \text{diametral product} (L \times B \times OH)$, where $OH = \text{auricular height}$.

- (i) Aino, ♂ and ♀, C and P_2 , *Phil. Trans.*, *loc. cit.*, pp. 234–5.
- (ii) Bavarians, ♂ and ♀, C and P_2 , *Phil. Trans.*, *loc. cit.*, pp. 235–6.
- (iii) Nagada Predynastic, ♂ and ♀, C and P_2 , *Phil. Trans.*, *loc. cit.*, p. 237.
- (iv) Negroes, ♂ and ♀, C and P , *Biometrika*, Vol. X, pp. 188–9.
- (v) Nagada Predynastic, ♂, C and P , *Biometrika*, Vol. III., p. 370.
- (vi) Theban Mummies, ♂, C and P , *Biometrika*, Vol. III, p. 370.
- (vii) English, ♂ and ♀, C and P , *Biometrika*, Vol. III, p. 370.
- (viii) Bavarians, ♂, C' and P , *Biometrika*, Vol. III, p. 370.
- (ix) Eskimos, ♂, C and P_2 , *MAN*, Vol. XXV, No. 97.

* *Anthropological Papers of the American Museum of Natural History*, Vol. V, Part II, pp. 179–280.

† See *Phil. Trans.*, Vol. 196A, pp. 243 *et seq.* *Biometrika*, Vol. I, pp. 460–1; Vol. II, pp. 347; Vol. III, p. 370; and Vol. X, p. 189.

‡ The English formula for arcual product was based on Macdonell's measurement of Q' through the bregma, while as a rule Q is taken through the apex, the point vertically above the auricular axis in the sagittal plane. Miss Hooke kindly measured 75 English male crania and found $Q = .9942 Q'$, so that correction is hardly needful.

§ An amusing incident may be connected with the formula given by Dr. Isserlis for negro male crania. That formula is based upon a variety of negro or negroid skulls. Mr. Pyecraft believes the Boskop skull not to be negroid, yet he applies this intra-racial formula to determine the capacity of the Boskop cranium saying (*J. R. Anthr. Inst.*, Vol. LV, p. 184) that he has received the formula from Dr. Duckworth—who, however, has no recollection of having provided it. However, biometricians must be grateful for all the mercies that flow from the hands of Mr. Pyecraft, and can afford to smile when they see him using and guaranteeing the reliability of a formula based on material of which he elsewhere (*MAN*, 1925, 117) emphasises “the absolute uselessness” for “the whole investigation.” And, again, *Nature*, Feb. 6, 1926: “I am quite satisfied with the formula I used in estimating the cranial capacity; and this formula has the approval of no less an authority than Dr. Duckworth.” There is nought so queer as folk!

(x) English, ♂ and ♀, C and P, Miss B. Hooke.

(xi) English, ♂ and ♀, C and P_1 , Miss B. Hooke.*

Now naturally we ought to pick out the race which we consider closest to the Eskimo. This, of course, is Mr. Dudley Buxton's own formula, which needs, however, a correction in sign. But supposing we had not that formula, which formula would be the best? The Eskimos are a markedly dolichocephalic race, with a long head and very small parietal breadth. We are not likely to get much accuracy from applying the Egyptian formula, nor are we likely to get much of value from Bavarian, or Negro intra-racial formulæ. Again, the Aino is a relatively short-head with a cephalic index much above the Eskimo. I confess that there is in the intra-racial formulæ so far worked out little akin to the Eskimo in race. But, encouraged by Sir Arthur Keith's statement (which I do not at all accept!), "that the Chancelade skull is in its essential character just as European as the people of England and France to-day," I shall boldly apply the English formulæ and see whither they lead, and this notwithstanding that I have not yet seen a fellow-countryman with a sagittal crest to match the man of Chancelade! Mr. Dudley Buxton's formula would give for the man of Chancelade a capacity of 1586.2. But I think the formula as it stands in MAN must be incorrect, for it does not give the right mean capacity for Fürst and Hansen's male Eskimos, if we substitute their mean length, breadth and auricular height, *i.e.*, $L = 188.4$, $B = 134.4$, $OH = 120.4$, for these give $C = 1456.0$ and not 1527.7. Even if we change the sign of the constant term we get 1531.0 and not 1527.7. If we suppose a slip of sign in Mr. Dudley Buxton's constant term we find for the Man of Chancelade $C = 1660.6$, which agrees well with the value 1650 suggested by Miss Tildesley in MAN, 1926, 2. She reaches this value by noting that Testut followed Broca's method, which undoubtedly gives values much too large, *i.e.*, anything from 60 cms.³ to 80 cms.³ excess. Further, the base of the skull was so damaged that accurate measurement of the capacity was impossible. A good reconstruction formula is likely to provide a much more reliable result than direct measurement. If we assume the Chancelade to be a *modern* Eskimo skull, then Mr. Dudley Buxton's formula (as modified) would make its capacity 1660.6. But suppose we do not consider it with Professors Testut and Sollas an Eskimo, but, like Sir Arthur Keith, suppose it just as European as a modern Englishman and apply our English formulæ for diametral products and for arcual product, *i.e.*, (vii) and (x) and (xi), what follows?

We have:

$$(vii) C = 219.2 + .02067 U \times S \times Q.$$

$$(x) C = 198.87 + .000366 L \times B \times H.$$

$$(xi) C = 247.86 + .000416 L \times B \times OH.$$

We find:

$$(vii) C = 1677.3.$$

$$(x) C = 1654.4.$$

$$(xi) C = 1626.1.$$

Or a mean value of 1652.6, which is not only in good agreement with the corrected value from Testut, but is only 8 cms.³ below the value from Mr. Dudley Buxton's formula for the Eskimo, if we correct that formula. Indeed Mr. Dudley Buxton's formula should, I suspect, be

$$C = 33.9 + .00049 (L \times B \times OH),$$

in which case his estimate for the Chancelade should be $C = 1657.3$, which is in excellent agreement with the result from the English intraracial formulæ. It does not appear to me that the problem of whether the Chancelade is Eskimo or

* From a paper now at press and dealing with a very large number of London crania.

modern European can be settled in any way by its capacity. The *norma lateralis* and the *norma occipitalis* are both in favour of its close Eskimo relationship, but, on the other hand, the *norma facialis* and the *norma verticalis* are by no means so favourable; one misses in the former the parabolic form of the upper part of the face and in the latter the characteristic "beetle" formation of the Eskimo.* These divergencies might lead us to suppose the Man of Chancelade to be indeed related to the Eskimo, but that the Eskimos have preserved a more primitive form of the stock from which both have sprung.

I trust this note will have made clear the following points:—

(i) The need for discriminating between the formulæ appropriate for the reconstruction of racial *mean* capacity, and for the reconstruction of the capacity of individual crania.

(ii) That Mr. Dudley Buxton's formula for Eskimo does not give a value for the Chancelade skull's capacity differing sensibly from that of the English intra-racial formulæ.†

(iii) That possibly the truth stands midway between the positions adopted by Professor Sollas and Sir Arthur Keith, namely, that the Man of Chancelade had an element of modernity in him, which is wanting in the existing Eskimos, but that he undoubtedly had Eskimo features which fail to be present in modern man.

One word more. Sir Arthur Keith tells us (MAN, 1925, pp. 186-9) that we must identify the racial nature of a given skull by applying to the task the method which Linnæus used for the discrimination of species. Unfortunately, Linnæus started his *systema naturæ* in regions where it is not possible to use the callipers. "A cast of the eye," Sir Arthur says, "is sufficient for a diagnosis in making a racial distinction of a man or of a skull;" and, further, he suggests that the most essential qualification for the equipment of the craniologist is a complete and intimate knowledge of the skulls of all races of mankind. Agreed, but the result of such doctrine seems to be that when two craniologists disagree, we should not appeal to numbers, which everyone can test for themselves, but we should measure the authority of their "cast of the eye" by reckoning up the number of the crania they have held in their hands. Is there not a touch of the mediæval Schoolmen in this doctrine?

I have to thank Mr. G. M. Morant for allowing me to use the measurements he has recently taken on the Chancelade skull and for the use of his reductions of Fürst and Hansen and other Eskimo measurements. I have also to thank Miss Beatrix Hooke for allowing me to cite her English diametral products formulæ.

KARL PEARSON.

Britain: Archæology.

Burchell.

The Origin and Evolution of the Epipalæolithic Axe; an Hypothesis. By J. P. T. Burchell.

28

The problems created by the reappearance of axe forms in Europe during the Epipalæolithic period and their subsequent development into Neolithic and Bronze Age times continue to produce numerous suggestions for solution. The object of

* This "beetle" formation approximates to, but is not, the *ovoides* of Sergi. It is quite visible in the *normæ verticales* provided by Fürst and Hansen, and is well marked in the cast of a typical Eskimo skull to be obtained from Dr. Krantz; it is less obvious in Hrdlička's Smith Sound *normæ verticales*, which are, however, not orientated, as Fürst's and Hansen's, to the Frankfurt horizontal and therefore not comparable.

† It would give a much worse result if the capacities measured by Fürst and Hansen are all in excess, as I suspect. With the mean Eskimo capacity of 1472 cms.³ provided by Duckworth, Bessels, etc., the Chancelade skull would hardly have a capacity above 1600.

this note is to view the position from a novel standpoint—a course suggested to the writer by reason of last season's work at Lower Halstow, Kent. It was not until the culmination of the Magdalenian phase in North Spain that the Capsian emigration from Africa, *viâ* Southern Spain, commenced to extend through France, establishing itself through the medium of the two contemporary microlithic industries of Maz d'Azil and Fère-en-Tardenois. The decisive factor in bringing about this cultural change should be attributed to climate. Weather conditions improved, with the result that the reindeer retreated northwards. The Magdalenians mostly followed. The few who remained would become wholly absorbed by the advancing Capsian stock, which also was spreading rapidly northwards as the snow and ice retreated. This process set up a series of "culture creeps," with the result that the further north the main body of Magdalenians retreated the more diluted became their stock, until a time was reached when Capsian influence predominated. But there would be a climatic limit to this northward migration and progress was ultimately brought to a stop in Southern Scandinavia. From the final amalgamation of Magdalenian and Capsian influences there developed the Maglemose culture, and with it the reintroduction of the axe. This hypothesis conforms with the established phases of art and artefact.

PHASE A.

Africa—North	-	-	-	-	-	} Capsian.
Spain—South and Central	-	-	-	-	-	

Implements microlithic, including the graver, and transverse arrowhead in its primary stage, showing a greater breadth than length. Bone used. Conventional art.

Spain—North	-	-	-	-	-	} Magdalenian.
France	-	-	-	-	-	

Implements non-microlithic, including the graver. Deteriorating in quality. Bone used more than flint. Bone harpoons. Naturalistic art.

PHASE B.

Spain	-	-	-	-	-	-	Capsian.
France	-	-	-	-	-	-	Azilian-Tardenoisian = Capsian.

Implements microlithic, including the graver, and transverse arrowhead in its secondary stage—that is to say, much bolder and becoming attenuated. Horn chisels, etc. Deer antler harpoons. Conventional art.

PHASE C.

Spain	-	-	-	-	-	-	Capsian.
France	-	-	-	-	-	-	Azilian-Tardenoisian = Capsian.
England	-	-	-	-	-	-	Azilian-Tardenoisian = Capsian.
Scandinavia—South	-	-	-	-	-	-	Maglemose.

Implements mostly microlithic—the transverse arrowhead in its third and final stage, now double its previous size. Horn axes and chisels. Bone axes, etc. Harpoons and barbed hooks. Naturalistic and conventional art. In spite of abundant supplies, flint was little used; horn and bone being preferred. Infrequently, there occur with this culture *shell mound* axes and picks: these are macrolithic. In size they correspond with the bone and horn chisels and axes, and they were instituted on account of their superior strength and cutting power. The shape of these *shell mound* axes was modelled direct from the existing flint chisel-ended or transverse arrowhead, whilst the pick form was a copy in flint of the heavy bone and antler points. With the climatic barrier still set against any further northward movement, the Maglemose culture would evolve itself into that of the

Shell Mound industry. Here we find that the microliths, the bone and antler chisels and axes, together with naturalistic art, have disappeared.

PHASE D.

Scandinavia—South - - - Shell Mound.

Flint implements (macrolithic) occur in large numbers—the *shell mound* axe, the pick and the transverse arrowhead predominate. Bone harpoons and barbed hooks. Conventional art. Pottery.

Both during and prior to this latest cultural evolution the Maglemose industry expanded southwards through England as far as North France. At a slightly later date, scattered along this same route, but penetrating a long way further south, we find remains of the *Shell Mound* industry. These two southward migrations from Denmark had to pass through territories still occupied by a microlith-using people, namely, the Azilian-Tardenoisians, sub-cultures of the Capsians of Africa. Thus it is not surprising to find at Thatcham, Berkshire, an industry consisting of *shell mound* and microlithic implements. From the *Shell Mound* period we slide almost imperceptibly into the age of polished stone.

PHASE E.

The Age of Polished Stone.

Judging by the types at Thatcham and Lower Halstow, the writer is of the opinion that this stage developed spontaneously along the southern line of migration through Scandinavia, England and France.

Thereafter the pendulum once again swings northwards, and for the dolmen period Belgium and France provide us with the following association; *shell mound* picks, transverse arrowheads, polished axes, microliths, leaf, tanged and barbed arrowheads; while Mr. Hazzledine Warren has shown the same sequence from occupation sites in Essex which, without question, are pre-Bronze Age in date. In Scandinavia, however, it is not until the development of the Bronze Age that the leaf shaped, the tanged and the barbed arrowheads first make an appearance.

J. P. T. BURCHELL.

Mesopotamia: Archæology.

Buxton.

Excavations in Mesopotamia. By L. H. Dudley Buxton, M.A.,
F.S.A.

29

When Mr. Field and I left Mesopotamia, the joint expedition of Oxford University and the Field Museum, under the direction of Professor Langdon, had been excavating for nearly a month on the old site at Kish. The area of the city is very big, and, although much has been excavated, large areas still remain untouched. While we were getting our workmen together, the Professor decided to continue digging on the site referred to in last year's report as "W." The site, it will be remembered, is Neo-Babylonian and badly preserved, possibly owing to destruction by fire; although, owing to the absence of baked bricks in most of the ruins at Kish, it is often, except in skilled hands, difficult to decide which is wall and which dust. "W" provided us with a series of small figurines, some burials in clay coffins, and a large series of tablets. I understand that those which have already been read presented no very remarkable features. The bones are not well preserved, but are of especial interest to the anthropologist because, apart from a few excavated at "W" by Talbot Rice and not at present published, none appear to have been obtained previously from this region of that date. Simultaneously with the work at "W," excavations were begun on the great mound of Ingha'ara, about half a mile away. The mound is a ruined *ziggurat* of very early date, at the base of which lie numerous and extensive mounds, some of which will

be cleared this year. On the surface, close to the base of the *ziggurat*, is a wall built of bricks of the Hammurabi period. The mounds were attacked from the side exactly opposite "W" by a series of parallel trenches running into the mound, and as soon as walls were discovered the area was cleared. We found a series of late buildings whose exact plan was not easy to ascertain at this stage of the excavations. Finally, we found a wall running parallel to the face of the big *ziggurat*. On clearing the top of this, a wall of plano-convex bricks was discovered, apparently inside the latter wall. Work was being carried out here when we left. Meanwhile, at a lower level, in between the walls, we found an early Sumerian stratum in which burials had been made. Here, fortunately, the soil was less solidified, at least in places, and some of the bones were well preserved. I was fortunate in being able to excavate a complete skeleton practically undamaged, and other graves were excellently preserved. The skulls belong to the extremely longheaded type, to which I have drawn attention in the report of excavations at Kish last year; but, owing to their state of preservation, it will now be possible to study the characters of the early Sumerians in greater detail than was possible before. While excavating one of these graves (which was entirely destroyed), I found a large vase, about 40 cms. high, of polychrome ware, with vertical and horizontal banding and naturalistic designs, probably palm leaves. This type of ware had not previously been reported from Kish, but, as it was only found just before we left, there is every chance of finding more.

In the meanwhile, the Professor, leaving Mr. Mackay in charge at Kish, had opened up a new site some fifteen miles away eastward. This site lay in a waterless region, and excavation was only made possible by the purchase of a Ford car. Painted pottery had been found last year at Bagutheit and is now in the Ashmolean, but this site was early abandoned in favour of the neighbouring mound Jemd-en-Naz'r. Here, working with a small gang, the Professor found a series of unbroken painted pots, and some clay tablets written in a linear pictographic script. The ware seems to resemble that of Susa II, and the writing is of the earliest type hitherto found on clay, but recalling the previously-found stone pictographic inscriptions. So little of the site had been uncovered when we left that the plan of the buildings could not be described; there appeared to be a series of rooms and a narrow passage. No graves have been found here at present, but I found traces of human bones weathered out on the surface of the mound that may belong to this period, so possibly more specimens await discovery.

Apart from excavations, we spent a considerable time searching for flint implements; numerous sickle blades, serrated on both edges, were found, and some finely-worked microlithic cores recalling those from Northern India.

The anthropologists of the party measured a large number of Arabs, both at Kish and Hillah. The figures have not yet been examined, but the old narrow-headed type certainly survives in the modern population, although there is a greater proportion of round-heads, especially in the town population, than in the early graves.

In crossing the desert between Amman and Ramadi in the Nairn Transport going out, we found a few worked flints. By the courtesy of the Air Vice-Marshal Commanding in Iraq, we were allowed to return with an armoured car patrol. We investigated numerous sites in the desert, and found worked flints were abundant in certain localities. The specimens are, unfortunately, somewhat atypical, but it would appear that some are Mousterian and others probably late Palæolithic; some, not unlikely, are Neolithic. All show a very considerable skill in flint-working and suggest a permanent occupation of what to-day is an area only occasionally visited by wandering tribes.

L. H. DUDLEY BUXTON.

New Zealand: Technology.

Firth.

Wharepuni: a few Remaining Maori Dwellings of the Old Style.**30**

By Raymond Firth.

A traveller over the rugged mountain trails of the Tuhoe country in the interior of the North Island of New Zealand finds much to interest him in the survivals of ancient culture still to be met with in the little Maori villages of the river valleys. Though not very apparent to the casual eye there yet remain *nga morehu*—fragments of old customs and ideas that were current among the Maori before ever the *pakeha* (white man) came with his novel ways of life and alien culture. For example, though so much alteration has taken place in the old way of living, it is interesting to find that there are still in existence a few *wharepuni* or dwelling-houses of ancient style.

The *wharepuni* is really a sleeping house, but folk will also sit therein on the cold winter evenings on account of the warmth and comfort. In the *Journal of the Polynesian Society*, Vol. V, pp. 145-154, will be found a very good description, by Archdeacon H. W. Williams, M.A., of the building of a Maori *whare*, but the house there described is not the usual type of dwelling which would be owned by the common people. It is a *whare runanga*, a meeting-house, a *whare whakairo*, a carved house such as would be the property only of a man of rank. The ordinary Maori house of the plebs was a much less pretentious structure and was also much more common. But its very numbers, combined with its squat character, rather mean appearance and general lack of distinction when compared with the carved house, has caused it to be somewhat neglected by the ethnographer in his writings, and no really detailed description of it exists. This is not the time for such a one, but a short *résumé* of its principal features will not be out of place. Hamilton in "Maori Art" barely refers to it.*

In general terms the Archdeacon's description will apply to the construction of the *wharepuni* also, if the more ornate characteristics, such as the carving, decorative reed panels, and painted rafters, are omitted. Even then there are constructional points of difference such as the absence of the *poutokomanawa* or central post of the ridge-pole. It must be borne in mind also that not all common houses were *wharepuni*. That term is restricted to the type about to be described.

First of all, a rectangular space is cleared, and the soil removed to a depth varying from that of a few inches to at times even as much as a couple of feet. The ground space of the house varies according to the accommodation required. Among the Urewera an oblong of about 14 feet by 10 feet is quite a common size. Two posts, about eight feet high, are then set up, one at the back, the other at the front of the *whare* to support the ridge-pole (*tahuhu*), often trapezoidal in cross-section, which projects out over the post in front. Dressed slabs (*pou*), about a foot wide and three or four feet high, are then set in the ground about 2 or 3 feet apart, as framework for the sides. To the tops of these the rafters are fitted, usually in a depression or slot, and run up to the ridge-pole, upon which their upper ends lie and butt to one another. Similar slabs (*epa*), graded in height to fit the pitch of the roof, are set likewise at the ends of the house. Small battens, a couple of inches thick and a few inches apart, are spaced in between the *pou* to act as lesser studs and serve as further support for the walls. There is no plate along the top of these "studs," but a batten near the top serves to hold them together and in line. The rafters are joined direct to the *pou* with an overhang of a few

* Since the above was written the second volume of *The Maori*, by Elsdon Best, F.N.Z. Inst., has appeared, and contains a few notes on the subject. But even Mr. Best has neglected the *wharepuni* in favour of the superior house.

inches to give a small eave. The two front rafters are faced by barge-boards (*maihi*) about 10 inches wide, and those again are supported near the lower ends by vertical *pou* rather longer than the others and facing out from the front of the house. The join of the barge-boards was often covered in the better class of *wharepuni* by a *tekoteko* or gable figure of carved design. This was the only piece of carving in a *wharepuni*. Neither *maihi* nor *pou* nor door lintel were carved, as in the case of the class of house described by Williams, or as was often done in the storehouses of finer workmanship.

In the front of the *whare*, space is left for a window and a door, both small, and it seems to have been the custom always to have the door on the right of the window, facing outwards. This is mentioned by Williams in his paper, and in both the houses figured here this is the case. The writer has seen many Maori *whare* of various kinds all over New Zealand, and cannot call to mind one in which this relative position of door and window is reversed. Even in quite European types, with those having but one door and window, this practice is generally followed. The door and window frames, especially the sills, are generally very solidly made. Against the supporting *pou* of the sides wide planks an inch or two in thickness are laid horizontally on edge one above the other and secured, to form the walls, and similarly in the case of the ends. These horizontal planks of the walls take the place of the *tukutuku* or ornamental reed-work described by Archdeacon Williams, Dr. Buck, and other writers as a feature of the larger *whare*. The inner wall is backed or lined to preserve the warmth, *raupo* (*Typha angustifolia*) or *ponga* (tree-fern) slabs being used for the purpose. Outside this again are set perpendicular slabs of wood which form the exterior wall, against which earth is banked up to still further retain the heat. It must be noted here that we are simply setting down the principles of construction of the *wharepuni*, not giving the necessary order of succession of the steps by which it is made.

The roof is formed by laying horizontal beams (*kaho*) on top of the rafters, about 12 to 18 inches apart, and then layers of *raupo* above, followed by long strips of totara bark laid right over the ridge-pole from eave to eave to turn the rain. This bark is kept in place by horizontal poles a foot apart, lashed to the *kaho*. The process of thatching a *whare* such as Williams describes was much more complicated. The roof and sides of all Maori houses projected out several feet beyond the doorway to form a kind of porch. A glance at the photographs shows the manner of this.

Such is, in short, the making of a *wharepuni*. The details of the process were not always quite the same, and the type of dwelling varied according to district, altitude and other factors, that of the Urewera being always built as far as possible to provide warmth and comfort in winter. Temporary houses in some tribes were built entirely of poles and *raupo*. The writer visited one of this type in the Northern bush as late as 1922 and admired its neatness. It measured about 20 feet by 15 feet and housed a family of five or six for several months.

A point which may be of interest to readers. Other writers have noted that the Maori always liked his house to face the sun. This can be corroborated even in the present degenerate days. Of half-a-dozen villages called to mind at random in nearly every case the meeting houses and principal dwellings face towards the east. All the *wharepuni* mentioned in this paper are so turned that the porch in front receives the full effect of the morning sun. The heaping up of the earth on both sides to conserve the heat explains why in nearly all descriptions of it the *wharepuni* is termed a warm house. The name itself is a further revelation, *whare*, of course, being a house, and *puni* in this connection implying blocked or plugged up. There are no means of ventilation except door and window, both of which

are extremely small, the sides of thick *raupo*, or it may be, *ponga* slabs act after the manner of insulators, and warmth is still further retained by the banked-up earth without. When the house is filled with people and a wood or charcoal fire is set going on the hearth, it will be readily seen that the houses merited the name of *warm*! There was no chimney, nor even a hole in the roof, and soot and smoke circulated freely. In any *wharepuni* that the writer has been in, of all, except the newest, rafters, ridge-pole and walls were stained a deep rich brown, varying in places to jet black according to age, the wall slabs presenting quite a glossy appearance from the resting against them of innumerable backs. The floor itself is merely earth, and, truth compels one to add, that a species of *Pulex* of the *irritans* variety were in a decided prominence in several of those visited.

A consideration of the photographs will give a clear idea of the general exterior appearance. The original of Fig. 1 stands in the settlement of Waikotikoti at Te Whaiti—the only example there. It is in quite good order, except that a portion of the *raupo* facing of the porch is torn away, exposing the battens beneath. Both

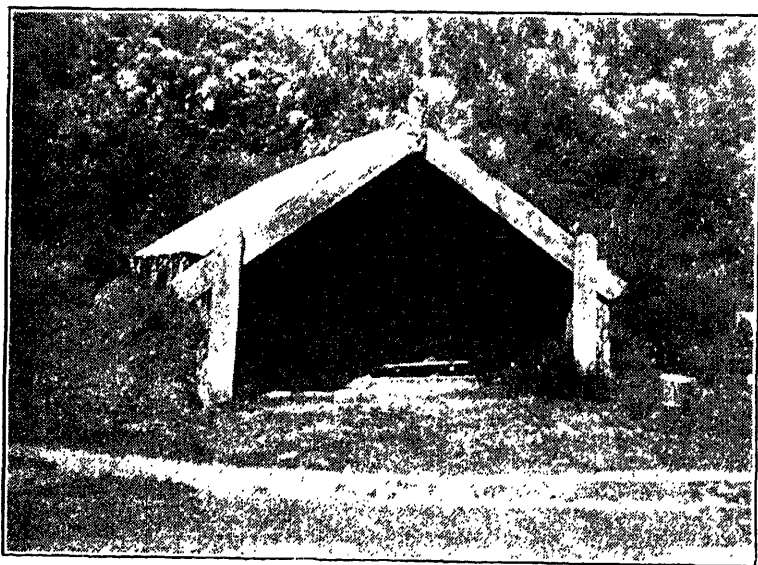


FIG. 1.—WHAREPUNI AT TE WHAITI

door and window are of wood, and slide back into the wall in rough grooves in the sill, being pulled to and fro by a flax string. The roof is of totara bark, but this has been covered over with corrugated iron, which, though some what offending to the eye, does seem after all a sensible attempt at

preservation of the house. Earth is banked up well around the sides, almost to the eaves. The *tekoteko*, the figure covering the join of the barge-boards on the gable, is well-carved and represents a seated figure clasping a conventionalised lizard in its three-fingered hands. One may observe in the photograph the universal tin bucket of the back-blocks. This *wharepuni* is in frequent use; in fact, persons were sleeping within when the photograph was taken.

In several of the *kainga* (villages) in the Vale of Ruatahuna similar dwelling houses are to be seen. A note of one or two will suffice. At Te Umuroa, a few months ago, there was one measuring about 13 feet by 10 feet, roofed first with a layer of totara bark, over which were laid shingles. Neither *maihi* (barge-boards) nor *amo maihi* (upright supporting slabs) were carved and there was no *tekoteko* on the gable. Earth was piled up around the sides as usual. The exterior front was composed of vertical slabs, about 10 inches in width, split, and dressed with the adze. Similar slabs (*pou*), about a foot apart, supported the rafters and the interior walls, while between them stood battens 2 or 3 inches wide. The space between inner and outer wall was filled with *kaponga*, the trunk of a species of tree fern. This lining was held in

place by strips of flax leaf as lashings which were passed back and forth and knotted to form a kind of quadrangular network pattern. The total thickness of the walls, exclusive of the banked-up earth, was about 11 inches, a bulk sufficient to retain a large amount of heat. The door was of just sufficient height to admit a crouching person; the window was about 18 inches by 15 inches. Both door and window were of wooden boards and were of the sliding type, running back on rough grooves into the wall. Adjacent to this building stood another *wharepuni* of larger size, measuring some 16 feet by 12 feet, of similar construction. Door and window were both larger and corresponded more with European ideas. This *wharepuni* possessed in the front of the porch a *paepae*, a massive timber in the nature of a threshold or sill. Both this and the door sill were about a foot high. It was in one of the two *wharepuni* that Tutakanguhau, chief of Tuhoe, lay in 1921 during the illness that preceded his death, and it was here one night that the Maori watchers around the sick man were struck dumb by the apparition of a *Kehua*—a ghostly visitant from the world of spirits. But of that there is not space to tell here.

Passing through Matatua recently the visitor might have noted a *wharepuni* under course of construction by Kokouri, the *rangatira* or chief of that settlement. When completed it will be of rather ornate type as the *pou* in the porch are graved with carving in the figures of *tipuna* (ancestors), a thing unusual in *wharepuni*. This one may have been intended as a *wharepuni runanga*, or small meeting house as well. One of this type stands at Te Wai-iti, some six miles from Ruatahuna, near the Waikaremoana trail. The carving was not recent, for, as is often the case with the Maori nowadays, the slabs had been removed from an old disused *whare* which had fallen into decay and one or two of them had been cut to fit their new abode. When the writer saw it the framework alone of the *wharepuni* had been erected, and in its uncompleted state it was of great interest as illustrating the method of construction and revealing, as it were, the skeletal features of such a house. The *tahuhu* or ridgepole was a large beam newly hewn, and shaped in cross-section thus :



Wharekiri, a well informed man of about 45 years of age, son-in-law of the head-man of Ohau-a-te Rangi, a village some eight miles down the valley of the Whakatane, was with me at the time and explained the method of construction, detailing the various parts of the building. Most of the names he gave need not be repeated here as they are common knowledge, but one or two are of some interest. Thus *heke* are the rafters running from the *pou* in the walls of the house up to the ridge-pole, while those rafters at the ends of the house were termed *ripi*. The name *heke ripi* is generally used for these. The post in front supporting the ridge-pole was in this instance called *pou tahuhu*. The *epa* were the upright slabs at the ends of the whare, while the *tumatahuki* were small battens alternating with the upright slabs round the sides and end. These are generally used as support for reed-work panels, but the term is evidently retained when, as in this case, no reed-work is employed. The lining was termed *kurupaepae*. The lining of the whare in this case consisted of wide dressed planks, laid horizontally edge to edge, and about 1½ inches thick. As *kurupae* means a beam or a joist it is probable that the former term is an extension of this and was applied not only to the bed-plate but to the lining boards above it as well. *Kiri* was used to indicate the roof of the whare and means primarily the totara bark of which it is composed. It may be mentioned in passing that *kiri* has many allied meanings. In addition to meaning the bark of trees it also signifies the skin of man. Also the inmost line of palisading which defended an old Maori fort was called the *kiri tangata* or warrior's skin. Yet again the *hapu*-sub-tribe—which dwells at Te Oputau is *Ngati-kiriwaeu*, though whether

or no this is any indication of the horny nature of the soles of their feet I am not prepared to say.

At Ohau-a-te-rangi, where dwell the Ngati-Rongo, the *rangatira* Waewae te Kotahitanga proudly introduced me to his dwelling, an excellent example of a *wharepuni* with *tekoteko*, totara bark roof and all complete. As is often the case with the more prized personal possessions of the Maori the house had a proper name assigned to it, being called "Te Koropu." (This name may mean either a hole or store, or more probably it refers to the building being of worked timber.) The boards were but roughly dressed, and the finish of the house was not such as a European carpenter would be proud of, but it was staunch and solid and kept out rain and kept in warmth, so what more could anyone want? The old chief gave quite a lot of information about the construction of his home and I give below some of the terms he employed. To Maori scholars most of these will be familiar, but it is of interest to record them as they may show points of local usage. Moreover, anthropology is a study of present as well as of past conditions, and it is important to know to what extent words—more especially technical words—are still retained by natives in current speech. These terms are contemporary linguistic facts and give a fairly reliable index as to the trend of culture and the amount of change that the language and ideas of the people have undergone. Consequently the citation of specific instances of the employment of words in vernacular conversation is not without its uses. The term *paitara* was applied to the walls and *tuaruna* to the back of the house, while the inside slabs were called *pou tahu*. (The old man may have been mistaken here: cf. *pou tahu* among other tribes as the post which supports the ridge-pole). The *whare* was lined between inner and outer walls with *raupo* leaves (*Typha angustifolia*), which when thus used in dry bundles are very efficacious in keeping out the cold which strikes so deeply in winter on those high wooded ranges. This lining or packing was termed *paru*. The window was called *puta auahi*, the doorway *te waha*, the lintel beam being called *tapatu*, the jambs *waewae*—a word meaning legs—and the lintel *pare* (the term *korupe* used by Ngatiporou and some Northern tribes does not appear to be employed by Tuhoe). In common with all Maori houses the space for seating and beds was around the sides, where upon the earth floor the mats were laid or rushes and fern strewn. Extending down the centre from the door was an oblong space a couple of feet wide and some six feet long, marked off by wooden planks set on edge, and here in the middle of this was the *tekuahi* (hearth or fireplace) made in a small hollow in the ground. Often three or four stones were placed on edge to form a small quadrangular enclosure, but in this instance one only was employed as a back-stone. The earth banked up against the outer walls was given no particular name, being merely termed "*He papa mo te whare*." The house was roofed with bark (*kiri*) which was held in place by beams laid longitudinally, consisting simply of untrimmed poles of *manuka*. The lashings holding them in place were of *aka-tea*, a particular species of supple vine which is specially prepared and used for the lashing of woodwork such as fences, staging, etc. The gable figure was of the type generally termed *koruru*, i.e., a carved head with projecting tongue, and was of quite fair workmanship. It was not painted, nor was any portion of the whare itself.

Fig. 2 shows a group of natives in front of the *wharepuni*. The old man is my informant, Te Kotahitanga, a chief of gentlemanly bearing and innate dignity, in spite of the fact that he goes barefoot and cannot speak a word of English. The old lady is his wife Te Hirea, a woman of simple kindliness, who also can converse only in her native tongue, while the three small fry are their *mokopuna* (grand-children). Two more of them, somewhat shy of the pakeha and his *whaka-ahua*—camera—(literally a likeness-maker) have shrunk back into the darkness of the porch. The old couple have each donned a feather cloak for the occasion, *Waewae*

being robed in a *kahu kiwi*—a flax mat covered with tufts of *apteryx* feathers). His wife has around her shoulders a mat adorned with white feathers of *kereru*=native pigeon, blue-black iridescent plumes of the *tui* (*Prothemadera novæ-zelandiæ*) and green and-red wing feathers of the *kaka* (*Nestor meridionalis*), the whole forming an effective lozenge pattern of alternate black and green-and-red on a white ground, with a border of grey *kiwi* feathers. In her hand the old lady holds a rather striking *taiaha* made of whalebone.

Other *wharepuni* are to be met with, though rarely, in the small villages amid the bush-clad fastnesses of the Urewera country; but

enough has been written to show their present method of construction and the extent to which they are used. In nearly every case they are the dwellings of the older generation, and these few observations have been put on record as an indication of a type of culture which is fast disappearing for ever. With the passing of the old people of the present generation the *wharepuni*, the dwelling of their forefathers from ancient days, will vanish and be seen no more.

RAYMOND FIRTH.

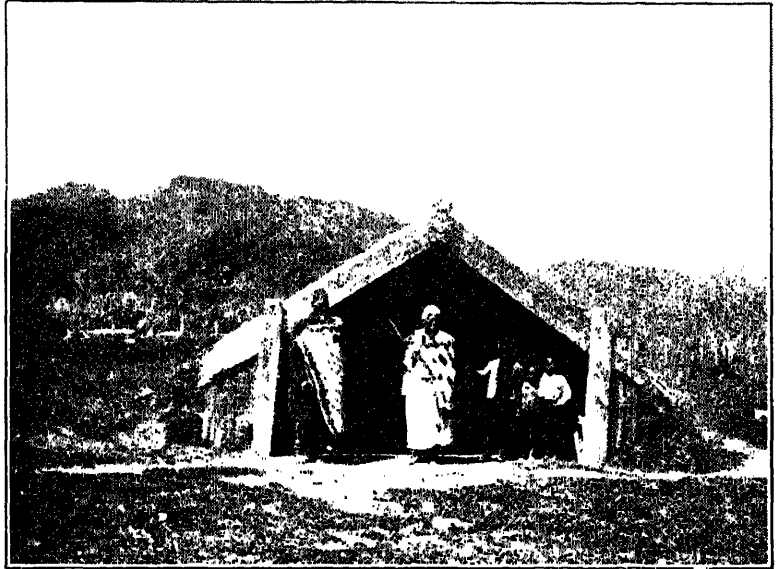


FIG. 2.—WHAREPUNI WITH BARK ROOF AT OHAU-A-TE-RANGI.

REVIEWS.

Archæology.

Ipek, Jahrbuch für Prähistorische und Ethnographische Kunst. Edited by Herbert Kühn; published by Klinkhardt & Biermann, Leipzig. 1925.

Kühn.

31

The editor is to be congratulated on his conceiving and producing this valuable and important publication, which one may hope is only the first of a long series. The study of a people's art is above all things important, whether the folk are still existing in out of the way backwaters of the world, or whether it is a case of prehistoric cultures long since extinct. In the latter case the archæologist is often perhaps too inclined to visualise a culture solely through the objects excavated and as the excavations consist very frequently of the "dust bins" of the people, a "kitchen" rather than a "drawing-room" view is obtained. A people's art enables us to understand and visualise many of their higher emotions: when the art is for art's sake, we can determine the artistic appreciation of a folk; when it is for sympathetic magic or other ritual purposes, we gather a faint impression of their religious emotion, that something which apparently so especially differentiates mankind from the rest of the animal world. Hitherto the enquirer studying the art, whether of prehistoric or modern primitive peoples, had to turn to the scientific accounts of the cultures, where the art is only treated as one among many studies;

but in the new *Year Book* art, removed from its other settings, is grouped together in a form that is particularly pleasing.

The *Year Book* is divided into two parts, to which representatives of several nations have contributed, each author writing in his own language, but with frequent summaries in English. In the first part various manifestations of prehistoric art are treated in a series of articles by a number of writers, while in the latter part of the book the art of modern primitive peoples is dealt with in a similar way, and it is pleasing to note here an article on Babunda (a Bantu-speaking tribe of the Belgian Congo) weaving by T. A. Joyce of the British Museum. In the earlier section there is a very excellent comprehensive account of the Palæolithic art found at the Cave of Lespugue (Haute Garonne) by St. Périer. It was from here that there came the well-known Aurignacian statuette of a woman found a short time ago, and it is excellent to possess now a large full-page beautifully executed illustration of this important "Venus." Passemard contributes another fully-illustrated account of the art found at the Cave of Isturitz (Basses Pyrénées). Important too is M. Breuil's article on the Late Neolithic painted birds from the rock shelter Las Figuras in the Province of Cadiz. It is of especial interest to the reviewer, as he was with Breuil early in 1914 when the original study was made. Las Figuras belongs to that remarkable, though not too well known, art group in South Spain that dates from the end of the Neolithic and the Copper Age periods. Dr. Obermaier has given us an enlarged edition, fully illustrated, of his work on the Bronze Age rock engravings in Galicia; and Italy has not been forgotten, as there is an important article on early prehistoric Italian plastic art. The latter section, besides Joyce's article already mentioned, comprises a number of important fully illustrated contributions, including an account of body painting and tattooing in South America, prehistoric pottery design in New Mexico, as well as many other interesting articles too numerous to give here, but no less important than those already mentioned. The book concludes with long *comptes rendus* of the latest important works, these being sometimes illustrated. Altogether the whole forms a nicely-produced, indispensable work for any one who is studying primitive art.

M. C. BURKITT.

Prehistory.

Ebert.

Reallexikon der Vorgeschichte, edited by Max Ebert. Berlin, 1924.

In progress.

32

A work which we think will prove of inestimable value to prehistoric archaeologists and of no small use to ethnographers has recently been initiated in Germany under the guidance of Professor Max Ebert, in cooperation with the enterprising publishing house of Walter de Gruyter. It aims at giving a conspectus of the extant prehistoric material—primarily from Europe—with the addition of summaries of comparative material from the early historic civilisations of the Near East and of the relevant ethnographic data derived from a study of modern primitives. Judging from the two volumes recently acquired by the Institute, this end is being satisfactorily achieved. The various articles on subjects such as Amber, Eridu, Finland, Fibulæ, Family, Aunjetitz Culture are accompanied by many excellent illustrations and a very full list of references, while the text itself is handy and easily assimilable, but at the same time thoroughly objective. This seems precisely what the archaeologist working in a special field needs, instead of hunting through innumerable scattered and often unintelligible periodicals to find the comparisons or other information he wants. Hence this encyclopædia is doing for prehistory what Pauly-Wissowa does for classical studies.

While admittedly, above all, the collective work of the leading German archaeologists, Ebert's *Reallexikon* is quite as international in its outlook as certain works

recently initiated elsewhere that lay claim to that title, and has so far successfully avoided the tendenciousness which infects some German prehistory. We also notice in the provisional list of contributors several leading Spanish scholars, such as Bosch-Gimpera, del Castillo and Obermaier; Finns like Europæus, Hackmann and Tallgren; and many Scandinavians, as well as Czechs, Magyars and Poles. At the same time we are sure that English authorities would be glad to cooperate in such a meritorious undertaking. Such cooperation would prevent an oversight such as the omission of the Hove cup from the account of the English amber finds. Nevertheless the English literature has been adequately cited. V. G. C.

Australia: Totemism.

Róheim.

Australian Totemism: A Psycho-analytic Study in Anthropology. By Geza Róheim, Ph.D., with an Introduction by M. D. Eder. London: Allen and Unwin. 1925. Pp. 454. 10" × 6½" Price 35s. net. **33**

An important work and one extremely difficult to review from the anthropological standpoint, since it is written in what is in effect psycho-analytical shorthand; it accepts the whole apparatus of psycho-analysis and attempts to apply this to the life and religion of the Australian savage. This, though a perfectly legitimate standpoint, is disappointing to the anthropologist, to whom at the present time the most important relation between the two disciplines is the question to what extent the facts or theories of psycho-analysis can legitimately be used to explain the beliefs and habits of the less advanced peoples. As to the answer, Dr. Róheim has no misgivings: "I am convinced that the historian of Anthropology in future ages will note three great years in our science: 1871 for *Primitive Culture*, 1890 for the *Golden Bough*, 1912 for *Totem and Taboo*, and it is only with the third that we have begun to see behind the curtain of the stage on which the great Drama of Mankind is acted. Psycho-analysis opens the road for a dynamic point of view; we see savage behaviour in the making, we see the moulding forces at work." It is not, then, with any doubt or hesitancy that Dr. Róheim comes to his problem. The most that the anthropologist who is not a psycho-analyst can do is to study Dr. Róheim's work and attempt to evaluate it from the standpoint of his own experience of savages and neurotics, and with that portion of the doctrine with which he is sufficiently familiar to make his opinion of any value.

Dr. Róheim places the origin of totemism in that period of human history in which man still lived in what has been called the Cyclopean family. Tribe, clan, and family, to use later conceptions, are one, consisting of a group of females and young males roaming over a restricted area under the leadership of a single adult male. Then comes the now classic conflict between old and young, terminating in the death of the old male; so far, then, there is nothing specifically psycho-analytic in the author's conception; moreover, we are spared the actual castration of the victim (so often put forward by psycho-analysts), a view which the writer has always held to be unnecessary and to be beset with theoretical and practical difficulties. Here Dr. Róheim seems to make a definite advance, as he does in making clear that the Cyclopean family is not to be associated with totemism as we know it now, so much as with an earlier relation of man and animal species, which he calls the proto-totemic complex.

This complex is "the projection into the environment of those unconscious concepts and feelings which have arisen out of the situation determined by the so-called 'Cyclopean' family, thus making certain animal species symbolically representative of the father-mother-etc., complexes. These proto-totemic organisations must have been local, were neither patri- nor matrilineal in the sense these terms are now understood, one animal species representing the

"horde, and more especially its leader" (p. 57). Later, in each horde this gave place to divergent groups—at first presumably only two—each with its representative animal, as the gradual check of uncontrolled impulses made it possible for the old and young generation of men to live together. Thus arose at least two animal species for each horde, Eagle-hawk and Crow probably representing those of the parent horde, from which later there branched off a number of units which may now be termed "tribes." Further, Dr. Róheim explains that "the widespread "therio-morphic elements of the initiation ritual on the one hand, and of "mythology on the other, are to be regarded as the divergent survivals of the "proto-totemic complex . . . which also survives in a more direct line in the "animal-named marriage classes" (p. 50).

As to the significance of the two great totemic taboos, the avoidance of (especially eating) the totem animal, and exogamy, these are easily understood if it be agreed that a taboo is an inhibited wish (as it so obviously is in many food taboos), when the totemic taboos correspond to the wish to eat the forbidden animal, and to have access to the women, also forbidden on one ground or another. The great difficulty to the writer is the early identification of the ancestral old man or old men of the recurring tragedy with a particular animal; it is no explanation to point to modern neurotics (and even healthy individuals) in whom some such father-identifications undoubtedly exist more or less strongly, for it is in the main these examples that have led to the idea of the identification of the old sire and the totem animal among savages. As far as the writer knows, there is no explanation "why"; but, as in science generally, certain facts suggest a theory as a working hypothesis, and the latter best explaining the facts, the theory is extended to embrace a fresh group of facts, in this case the avoidance of the totem animals, which is now explained as the negative or inhibitory result (due to ambivalence) of the eating of the old man by the victors. Whether accepted or not, this is a perfectly definite idea, supported as it would seem by the Alcheringa (literally "dream-time") beliefs, myths of the good old times when men ate their group animals and had connection with their group-women without sin. The suggestion then is that the Alcheringa myth reflects the proto-totemic complex, due largely to the territorial identification of man-group and animal, before a long series of prehistoric parricides transformed a positive relation of man and animal species into a negative attitude of respect and avoidance due to repression as one aspect of the emotions set up by the killing, rather than the other, became dominant. This conception leads us to what is, perhaps, the most ingenious section of the book, that concerned with the *intichiuma* ceremonies. Briefly, the *intichiuma* is a relic of the proto-human breeding season, i.e., a time when man, at any rate Australian man, had a true rutting season; the magical elements of the *intichiuma* ritual being reduced survivals of connection which at that early date was regarded as a pre-requisite of fertility. The significance of the act, at first regarded as concerned only with human fecundity, was subsequently projected into the natural environment, i.e., linked with the annual proliferation during the "rains," with which it must have coincided. The closest association would thus be set up between the season of connection and the increase of edible plants and animals. For it must be remembered that in Central Australia the seasons are limited, so far as the breeding of animals and the flowering of plants are concerned, to two—a dry one of uncertain and often great length, and a rainy one of short and often irregular occurrence. The latter is followed by an increase in animal life and an exuberance of plant growth which, almost suddenly, transforms a sterile waste into a land rich in animal life and gay with the blossoms of countless flowering plants. The present form of these ceremonies with their extraordinarily complicated magical content are not, of course, primitive; elaborations have

arisen in a later stage when, with domestication (or relative civilisation) man acquired the faculty of propagating his species at all times, the great breeding festival being retained in its ceremonial aspect when the dancing and other spectacular forms of courtship (or, to use a psycho-analytical term, the fore-pleasure phases of the sexual act), no longer attached to the act itself, still retained the periodicity which the act had lost. Yet, as they had formerly led to coition and multiplication, so they were still regarded as effective, and continued in fecundity ceremonies. All this seems reasonable enough, but it is at first sight less easy to agree that the *intichiuma* "is really a repetition of the totem-father's mourning feast" (p. 233) unless it be realised that the first act after the killing of the old sire would be a rush for the hitherto forbidden women of the horde; this, in turn, being succeeded by those ambivalent feelings of awe, discomfort, and perhaps dismay, which we know are so often felt by homicides (even among ourselves) and among savages necessitate the numerous ceremonial forms of isolation and abstinence described in our text-books.

Taking the Kangaroo *intichiuma* of the Arunta (described by Spencer and Gillen, "The Native Tribes of Central Australia," pp. 204) as an example, Dr. Róheim works out this idea as follows, the writer slightly rearranging the order of his material, and the passages in italics being those from Spencer and Gillen which occur in his argument:—

"In the case of the Kangaroo totem of Undiara, after the men have allowed the blood to flow out of their arms over the stone ledge they descend, and after rubbing themselves all over with red ochre return to the main camp [. . .] All the younger men then go out hunting kangaroo, which, when caught, they bring to the older men who have stayed in camp. [. . .] The old men of the totem, the Alcheringa being in the middle of them, eat a little and then anoint the bodies of those who took part in the ceremony with fat from the kangaroo, after which the meat is distributed to all the men assembled. The men of the totem then paint their bodies with the totem design or Ilkinia, in imitation of the painting on the rock at Undiara, and that night is spent in singing about the doings of the Alcheringa kangaroo people and animals. Next day the ceremony is repeated [this sentence is a perfectly fair condensation . . .] After this the animal is eaten of very sparingly by the Kangaroo men, and they must on no account touch the choice bits [again a fair condensation]. The blood-letting of the elders and performers is an acknowledgment of and a self-punishment for having caused the death of the Alcheringa ancestor. By the blood-letting, however, they also engender the animal from the stone, and thus in a certain sense of the word they become the fathers of their own fathers. Painting the totem design on their body and singing the totemic chant is a clear sign of identification; we shall conclude that this absolute identification must be a reaction formation after a period of absolute though unconscious rebellion. Both the young and old men have previously done just what is prohibited for a Kangaroo man to do: they have hunted the kangaroo and they have eaten it. The elder and the younger generation have acted seemingly in absolute harmony; we find that both have given free vent to their repressed wishes. For have not the young men hunted the kangaroo, the symbol of the father? And have not the Elders eaten of the animal, which to them also is, in a primary sense, a Father-symbol, but which they have also procreated by letting their own blood drip on the rock, and which, therefore, from the retribution point of view of the Father, means the Son? We now see what the eating of the totem means in all these ceremonies: it is the necessary preliminary step for the rite of magical procreations, just as breaking through the inner inhibitions, the endopsychic taboo must precede the free flow of the libido that is necessary for actual procreation."

It is hoped that the above gives a fair view of some at least of the more important ideas in Dr. Róheim's book, which, from the reviewer's standpoint, is perhaps rather overloaded with detail, though this will make it the more valuable in the future. No *critique* can do justice to the erudition of the author, while the sincerity and honesty with which the volume is written deserves all praise. It remains only to express the pleasure the writer feels that so important a work should have been written in English (for it is not a translation), and should have appeared in this country, as well as to advise readers to give full weight to Dr. Eder's graceful and philosophic introduction. C. G. S.

CORRESPONDENCE.

Egypt: Archæology.

Petrie.

*To the Editor of MAN.***The Badarian Civilisation.**

SIR,—Mr. Burkitt's remarks on the Badarian question hardly apply to the present position, as such an entirely new opening naturally develops on enquiry. As the dating depends on new facts entirely outside of previous chronology, so the past debate does not therefore affect it. Nor would I state any equivalence with the Solutrean date as a proof, but as showing that there is no known discrepancy of ages. Regarding the types of flint work, I must say that the character of the forms in the Badarian work is very closely that of the Solutrean; there are only small differences of method, which are sure to arise between peoples who drift thousands of miles apart.

The evidence at present suggests that a long-standing Asiatic culture, which had attained to making fine pottery, ground stone tools, glazing, and a little native copper, then threw off branches repeatedly, which passed both west and south, at intervals of a few thousand years. Such branches lost much on their travels, and the long western trek brought them down to the condition we know as Solutrean. I regret that there has been no opportunity of discussing with Mr. Burkitt personally, which I still hope may be arranged.

FLINDERS PETRIE.

Technology.

Chinnery.

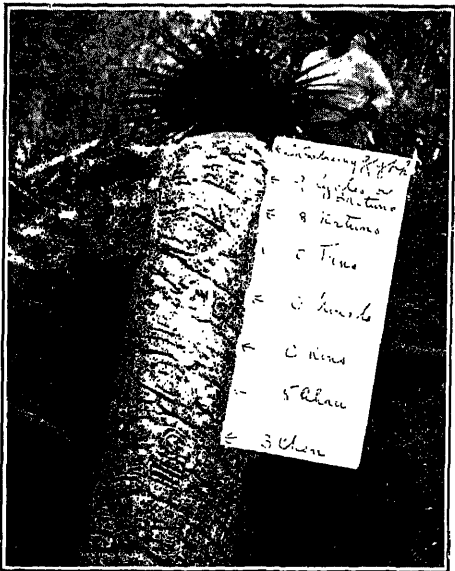
*To the Editor of MAN.***Thorn-lined Fish Traps.**

SIR,—I have seen the thorn-lined fish traps of calamus (Henry Balfour, MAN, 1925, 21) in Morigi Island, Turama river estuary, Papua, and in the village of (Mindimbit), Sepik River, Territory of New Guinea. In Mindimbit it was known as *chumis*.

E. W. P. CHINNERY.

ANTHROPOLOGICAL NOTES.

Lectures on the Study of Backward Races.—Professor A. Radcliffe-Brown, who has now relinquished his appointment of Professor of Anthropology in the University of Cape Town on his appointment to the newly-instituted Chair of Anthropology in the University of Sydney, will arrive in England in the course of the current month. Before proceeding to Australia, he will deliver a course of three Public Lectures at the London School of Economics, Houghton Street, Aldwych, W.C.2, on 10th, 12th and 15th March, at 3 p.m. The subject of the lectures will be "The Study of Backward Peoples: its Method and Practical Value." Admission to the lectures is free and without ticket.



NEWLY DISCOVERED STELA WITH HIERO-
GLYPHIC INSCRIPTION FROM QUINTANA
ROO, YUCATAN.

Initial Series Date
26 Oct 333 AD.

- | | |
|--|-------------------------------|
| | (1) Introducing
glyph |
| | (2) 9 Cycles |
| | (3) 8 Katuns |
| | (4) 0 Tuns |
| | (5) 0 Kinals |
| | (6) 0 Kins |
| | (7) 5 Ahau |
| | (8) 3 Chuen
(on left side) |

9.8.0.0.0. 5 Ahau 3 Chuen

THE INSCRIPTION.



THE INITIAL SERIES AS FAR AS THE TOP OF
THE AHAU SIGN.



GROUP OF UNKNOWN GLYPHS AT BASE OF
STELA.

A NEW MAYA STELA WITH INITIAL SERIES DATE.

ORIGINAL ARTICLE.

With Plate D.

America, Central.

Gann.

A New Maya Stela with Initial Series Date. By Thomas W. Gann, **37**
F.R.G.S., F.R.A.I.

The stela shown in the photographs was discovered by me on 6th January, 1926, on the east coast of the Chetumal Bay in Quintana Roo, Yucatan, while searching for Maya ruins along the coasts of the bay, which are completely covered with forest down to the sea shore and, with the exception of a few huts of Maya fishermen, entirely uninhabited. It consisted of a block of shale 9 feet long, 18 inches broad, and 12 inches thick. Most fortunately, it had fallen upon the side bearing the Initial Series, which had consequently been protected from the weather, and was found to be in a remarkably good state of preservation. The first glyph—which, owing to the slope of the stone, does not show well in the photographs—is undoubtedly the Initial Series, introducing glyph, superfix, katun sign, and trinal subfix, being easily identified. The second glyph is the face variant for the Bactun, showing the hand in place of a lower jaw. The numerical coefficient of this glyph shows half a bar and two dots, the upper half of the bar and the upper two dots having been broken away. It was unmistakably 9. The third glyph is very clearly the face variant of the katun sign, with the numerical coefficient 8—a bar and 3 dots; the fourth is the face variant for the Tun, with fleshless lower jaw and numerical coefficient zero. The fifth, the Uinal sign, like the full figure glyphs at Quirigua, exhibits the entire body of the frog, with projecting feet; the zero sign placed in front of the face. The Kin sign is badly damaged, but its coefficient zero is unmistakable. Then follows the day 5 Ahau, and immediately beneath this the month 3 Chen. The complete Initial Series then reads 9.8.0.0.0., 5 Ahau, 3 Chen, corresponding in Spinden's correlation to the date 26th October, 333 A.D. This makes the fourth Initial Series date ever found in the whole peninsula of Yucatan, and if a contemporaneous date, as it appears to be, is $14\frac{1}{2}$ katuns, or nearly three centuries, earlier than the next earliest date known in this region, at the great ruined city of Chichen Itza, which is 10.2.10.0.0. This remarkable find would, in fact, appear to indicate that the Maya occupied the south of Yucatan centuries before they were supposed to have deserted their southern cities of the Old Empire, and may necessitate a complete revision of our present ideas as to their migration into Yucatan, and the founding there of their New Empire. The stela stands a few yards to the east of the central of three vast, stone-faced, terraced pyramids, which are contained within a great semi-circular wall or fortification, with an arc of about $1\frac{1}{2}$ miles formed by the sea coast. The height of this wall varies from 3 or 4 feet, where it has fallen; to 12 feet where it is intact, reminding one strongly of the fortified cities of Tulum and Mayapan, the former on the coast, the latter in the interior of Yucatan. The whole site is covered with dense bush, but further exploration here is urgently called for to elucidate the mystery of this truly incomprehensible date. The region must at one time have been densely populated, as it is simply teeming with such small objects as potsherds, clay beads and heads, both animal and human, malacates, obsidian knives, flint chips and javelin heads, etc., which are to be found everywhere lying on the surface of the ground when it has been cleared of bush and undergrowth.

T. W. GANN.

Italy: Archæology.

Burchell.

A Fresh Discovery at Barma Grande. By J. P. T. Burchell. **38**

During a recent visit to the Riviera I had the good fortune to be granted facilities for studying, *in situ*, a newly discovered cultural horizon of the

Chelles or St. Acheul period in the Barma Grande, a cave situated slightly to the east of the Grotte des Enfants, Mentone. This stratum occurs 3 feet 6 inches below the Moustier base level and consists of cave-earth (the total depth has yet to be ascertained) in which have been found the following:—

1. Hearths.
2. Bones of *Elephas antiquus* and *Rhinoceros merckii*. [It should be remembered that in these southern latitudes their remains are found in strata as recent as those of the Aurignacian period.]
- and 3. A series of implements comprising for the most part heavy hand-axes formed from large quartzite pebbles, whilst the remainder are rough quartzite flake implements, some of which, from their shape, might well be mistaken in date for Moustier.

The heavy hand-axes (of which Fig. 1 is an example) are formed by boldly working up one end of a quartzite pebble.

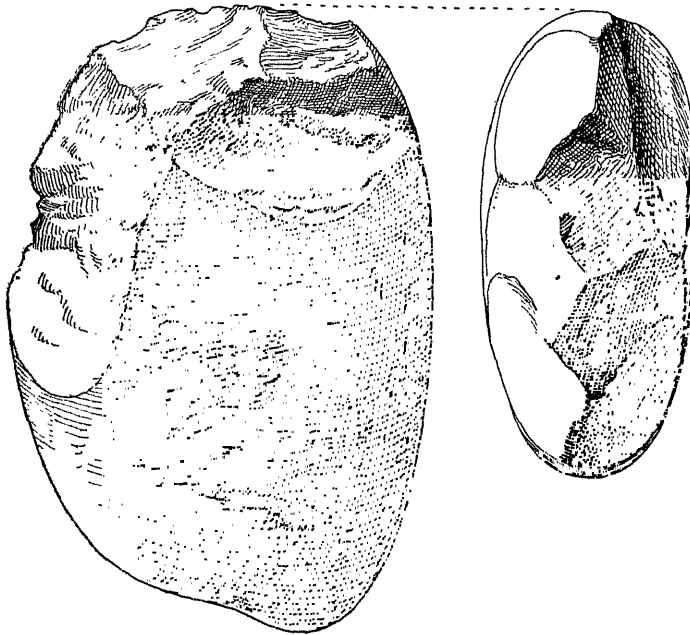


FIG. 1.—Scale 2/3.

In some instances I have seen both opposing ends so trimmed. The resulting cutting edge is zig-zag, a feature typical of Chelles times. Similar hand-axes have been recovered from Cresswell Crags. Implements of the Chelles and St. Acheul periods were found at Kent's Cavern so long ago as the sixties of last century, whilst an admirable series was found in the Abri Bourges - au - Moustier some eighteen years since, so that

news of these fresh finds at Barma Grande, important as they are, need cause no surprise.

The results, however, of Signor Lorenzi's patient work should turn our attention to questions of Palæolithic classification. It is made evident how misleading and useless are the terms "Drift period" and "Cave period" when used as Palæolithic subdivisions. So used the two are synonymous. The words "Drift" and "Cave" may be employed to denote provenance, but, in these subdivisions, never space of time.

J. P. T. BURCHELL.

Africa, Central: Dreams.

Hodgson.

Dreams in Central Africa. By A. G. O. Hodgson.

The following information was obtained from ten headmen of the Yao, Ngoni, Nyanja and Chewa tribes in Dowa District, Nyasaland, having regard to the questions suggested by Professor Seligman in MAN, 120, 1923. Two of the informants profess Mohammedanism, two had attended Mission schools for short periods, and all were

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middle-aged or elderly. Although questioned separately, they gave the same interpretation in most cases.

A person dreams because he has a spirit which survives after his death; if he did not dream, he would have no spirit and would perish utterly. No instance is known, however, of a man or woman who has never dreamt; many dream regularly every night in the cool season, and all do so more frequently in cold than in hot weather. Their dreams are usually of common occurrences in the everyday world, but occasionally also of connected stories with obvious meanings or with confused and nonsensical content. No contributory cause is alleged, but dreams of the latter classes have generally accepted meanings, and dreams are generally referred to only in cases where the dreamer is ignorant of the interpretation and seeks the advice of an older man or woman. Some such interpretations are as follows:—

(1) *Flying*.—If one dreams that oneself or anyone else is flying in the sky like a bird, the person flying will enjoy long life and good health.

(2) *Fire*.—If one dreams of a fire burning, the dreamer will be involved in a serious misfortune, generally in a law-suit. If the fire goes out, the dreamer will emerge from his suit satisfactorily, the rise and decline of the fire symbolising the outbreak and subsidence of the case. To dream of a great bush fire augurs the advent of war, but if ashes and smoke only, without flame, are observed, the war will come to a speedy and satisfactory conclusion.

(3) *Climbing*.—To dream that oneself or another is climbing a tree or ascending a hill signifies that the climber will be promoted to chieftainship or other high rank.

(4) *Loss of a tooth or teeth*.—Is taken to indicate that the dreamer will shortly lose his wife or child or other near relation. One (Chewa) informant interpreted this dream to mean that the dreamer's wife would bear a son who would grow up to be a strong man.

(5) *Oedipus*.—If one dreams that one is having connection with one's mother or sister, the dreamer is being bewitched by some unknown person, and may procure medicine to put in his house to catch his enemy, though usually he is too ashamed to take any action or to mention the matter.

(6) To dream of a flood of water in a river has the same signification as in No. (2), though the informant quoted in No. (4) stated that it might also mean that the dreamer's wife was commencing her period.

(7) To dream that someone is sick means that the person dreamt of will not be ill for a very long time, and to dream of the death of someone who is in reality sick at the time of the dream augurs the speedy recovery of the patient.

(8) A dream of frequent occurrence is one of being chased downhill by a lion, the common symbol for chieftainship. If the dreamer escapes, he himself will become a chief or otherwise rise to importance: but if he is seized by the lion, it means that a chief is plotting against him.

(9) To dream of crossing a river signifies death; but the informant quoted in Nos. (4) and (6) gave the same interpretation as in No. (2), the dreamer winning his case if successful in crossing the river.

(10) To dream of digging a pit or of hoeing *nthumbira* (raised mounds for maize or potatoes) signifies that the dreamer will soon be digging a grave for one of his relations, the pit being suggested in the second instance by the depression between the mounds.

(11) To be afflicted with lice is a lucky dream, as the dreamer will acquire great wealth, or, if hitherto a childless woman, will produce a son.

(12) Rain symbolises mourning at the funeral of a relation.

(13) To dream of a snake round one's leg means that the dreamer will be bound in prison, or, in former times, as a slave.

(14) Drinking beer symbolises the drinking of *mwabvi* (ordeal poison).

(15) To dream of falling through space signifies approaching sickness, but if the dreamer rises after the fall, he will duly recover.

(16) To dream of a hearth stone (*fua*) means that one will shortly see a chief, as the hearthstone always remains in one place, like a chief, and is not thrown away after use.

(17) If one dreams of an ant-heap, one's wife is pregnant by another man, the likeness being due to the fact that an ant-heap is always slowly increasing in size.

(18) If one dreams that one is catching fish, the dreamer will find a bag of money; but if the fish are of a slippery variety, like mudfish, the dreamer will not be able to keep the money, which will soon be lost or stolen.

(19) It is unlucky if one of the objects of the dream is confused or grotesque, as, for example, if a person or animal has some of the characteristics of another. The spirit of the dreamer is troubled, and must be appeased by the dreamer washing his person in medicine made from the roots of the *mtsizi* tree and by a beer dance.

(20) If one dreams of a dead relation wearing a black cloth, the dreamer will be in mourning for a long time owing to continual deaths of his relations.

A. G. O. HODGSON.

Anthropology: Physical.

Sollas.

The Chancelade Skull. By Professor W. J. Sollas, Sc.D., F.R.S.

40

By some mischance, I overlooked Sir Arthur Keith's remarks (MAN, 1925, 116) in reply to my criticisms in an earlier number and it was not till last month (February) that my attention was directed to them by a friend. Miss Tildesley's contribution I had seen a few days earlier. This and an unusual pressure of work must be my apology for a rather belated reply.

First allow me to thank Miss Tildesley for informing us of the formula actually made use of by Sir Arthur Keith, and, next, to express my deep regret that I was led to erroneously attribute to him another. In offering my apologies to Sir Arthur Keith I ought to add that the mistake in no way affects my argument, which indeed is sustained and emphasised by Miss Tildesley's interesting dissertation.

I do not propose to offer any rejoinder to such remarks of Sir Arthur Keith as are purely personal, but there is just one point to which I think I ought to refer. This is not, as that distinguished anatomist seems to insist, simply a difference of opinion between him and me. Nothing could be farther from the truth. Testut's masterly description was published in 1889 and ever since then the eminent anatomists who have given special attention to the Chancelade skull have been agreed on accepting its close relationship to the Eskimo's.

In the latest number of *L'Anthropologie* (Tome XXXV, Nos. 5 and 6, p. 599) Professor Boule writes: "Depuis le travail si complet et si minutieux de Testut, tout le monde avait adopté les conclusions de notre éminent et regretté anatomiste sur les affinités du squelette de l'Homme fossile de Chancelade avec celui des Esquimaux actuels."

Thus it is Sir Arthur Keith "*contra mundum*."

In approaching the facts I choose first the most important. Sir Arthur Keith makes two positive assertions: (i) that the two sides of the lower jaw are of the same form and size; (ii) that a slight error was made in repairing a fracture of the left half.

It is true that the right half was broken, indeed, in two places; the fractures are indicated on Plate XIII, Fig. 1, of Testut's memoir. But the work of repairing this and the whole of the skull was performed by Testut himself, and with the result of his work he was so well satisfied that he was able to write, while noticing some slight deficiencies: "La mandibule est parfaitement conservée." This is a definite assertion made by an anatomist who was distinguished by his precision of statement. I have given much attention to the mandible myself and am convinced that

the clean fracture near the symphysis has had no effect on the slope of the ramus.

But even on Sir Arthur Keith's own hypothesis, stated as a fact, we are led to conclude that the biangular diameter which the jaw now possesses is abnormal, and, indeed, he is himself led to admit that its original value may have been, as I have suggested, 100 mm., and this brings it within the range observed in the Eskimos, which for this male jaw is from 95 mm. to 139 mm. and only 10 mm. short of the mean which, as observed by Fürst and Hansen, is 109.7 ± 0.65 mm.

Of the first statement I can write very positively. The ascending ramus of the right side is far from being "of the same size and form as that of the left," as will appear when my full account of the skull is completed. But here again I must refer to Testut. He writes (p. 49 of his memoir): "Il n'est pas sans intérêt de faire remarquer que cette excavation de la face externe de la branche est beaucoup plus marquée du côté gauche que du côté droit: la branche droite est, en effet, presque plane. De plus, l'étude comparative des deux branches, faite le compas en mains, nous montre que la branche droite est à la fois un peu moins haute et un peu moins large que la branche gauche. Si nous rapprochons de cet fait que la fracture de la région temporale siège du côté droit et que la région temporale droite est actuellement moins développée que la gauche, nous pouvons admettre, sans dépasser les limites d'une déduction rationnelle :

" 1° Que le vieillard de Chancelade, à la suite d'un traumatisme qui lui brisa le côté droit du crâne et le priva de son muscle temporal droit, s'habitua peu à peu de ne mastiquer que du côté gauche.

" 2° Que les muscles masséter et temporal du côté droit, ne fonctionnant moins, se sont peu à peu atrophiés, entraînant comme conséquence une atrophie parallèle des régions osseuses sur lesquelles ils s'insèrent et qu'ils sont destinés à mouvoir."

The characteristic "jib-like nasal spine" of the Chancelade skull is clearly present in several Eskimos' skulls in our Museum Collection. There may be other differences between the Chancelade and the Eskimo's nose, but in this particular there is none.

In the Chancelade skull which I have examined the temporal muscles have an extension quite comparable with that presented by the Eskimos, but I am beginning to fancy that the Chancelade skull of Sir Arthur Keith cannot be the same as that which is known to me.

I must defer any reference to the other supposed *differentia* dwelt upon by Sir Arthur Keith till I am able to complete my promised memoir on the Chancelade skull.

W. J. SOLLAS.

Africa, South: Religion.

Sayce.

Lightning Charms from Natal. By R. U. Sayce, M.A.

41

A piece of dolerite, which appears to have been artificially shaped, was recently picked up, on the ridge on which the Natal University College stands, overlooking the town of Pietermaritzburg, by one of my students, M. W. R. Evans. The stone was shown by Mr. Edward Bird to one of his native servants, who comes from near the Umzimkulu River, in the south of Natal, and who immediately described it as a "lightning stone." I was able to obtain the following information from the native, thanks to the help of Mr. Malcolm, of the Education Department, and of Mr. Faye, of the Native Officers' Department, who kindly consented to act as interpreters for me.

Toward the end of winter, or in early spring, when the winter anti-cyclone is breaking up, and just before the first storms are expected, the natives take precautions to protect the huts from lightning, which is often of great severity, and is responsible every year for a considerable number of deaths. The Umgoma, or witch doctor, is called in, and receives a preliminary fee of £1.

The first step in the procedure is to collect a number of wooden pegs, which will be treated with medicine. One of the pegs, however, is generally of stone, though it may be of wood if no suitable stone is available. Whenever possible this "lightning stone" is obtained from a hill top, at a spot where lightning has struck and has fractured the rock, so as to produce a suitably shaped fragment. Sills of dolerite are very numerous in the area, and frequently cap the flat-topped hills. Moreover, they appear to attract the lightning. Dolerite is more likely to fracture into requisite shapes than either the sandstones or soft shales, of which much of the rest of the country is formed. Consequently it is probable that most lightning stones are of dolerite.

The pegs are now dressed with a medicine, consisting of three ingredients. Two of these are barks of different trees, which I was unable to identify; one is a small tree, or shrub, with very long thorn. The second bark exudes a red sap, said to resemble blood. The third ingredient is the fat of *i Ngqungqulu*. This word is translated in Bryant's Dictionary as Bateurs, or the Tumbler Eagle (*Helotarsus ecaudatus*). The bird, when flying quickly, makes a noise like thunder. With the fat of this bird may be mixed the fat of a "peacock,"* and the whole is then boiled.

The Umgoma then takes the wooden pegs and drives them into the ground at intervals around the hut, but at some distance from it. As lightning is supposed to enter the hut by the doorway, the "lightning stone" is placed just outside the hut, at the foot of the right doorpost as one enters. A hole is first made to receive the stone, and into the hole is put a charm, which is made by grinding up a bark and mixing it with a glistening ore resembling argentiferous galena, obtained from a spot on the Ingeli Mountains which is frequently struck by lightning. The object of this charm is to prevent the lightning stone from being knocked out of the ground by the lightning.

The stone is next placed in position and left with an inch or two of its end projecting above the ground. Over the stone is now spread a third mixture, consisting of a glistening red ore† and cows' milk.

After the pegs have been treated and placed in position the Umgoma receives an ox. This is the last payment made; but every year he has to revisit the pegs and give them another dressing with the same medicines.

R. U. SAYCE.

Britain: Archæology.

Moir.

Man and the Ice Age. By J. Reid Moir.

42

In the *Summary of Proceedings* of the Prehistoric Society of East Anglia (Vol. II, pt. 1, p. 156) is recorded the discovery at Eccles-on-Sea, Norfolk, by Mr. W. W. R. Spelman, a well-known resident of Norwich, of "a well-shaped Palæolithic ovate, $5\frac{3}{8}$ inches by $3\frac{3}{8}$ inches, with lustrous black surface, found by him on the beach at Eccles-on-Sea, Norfolk, 33 yards due north of the remains of the ruined church, about half-way between high and low tide-mark, lying in a bed of flint stones."

During the present year, my friend Mr. J. E. Sainty, who has afforded me valuable help in my researches on the north-east coast of Norfolk, drew my attention to the above-mentioned specimen, and it has been through his good offices, and the kindness of Mr. Spelman, that I have been enabled to make an examination of this important flint implement, which is here figured (Figs. 1A and 1B). The specimen exhibits the following characteristics: (a) it is made from a more or less

* The native stuck to the term "peacock" when cross-examined, but I cannot identify the bird. I was told that it does not occur in Natal, and is rare to the south of the Umzimkulu. It cries, and ruffles its feathers before thunder.

† A very small fragment of this ore appeared to be cuprite.

tabular-shaped mass of flint, and, as areas of the white cortex appear on either side, it is not formed from a flake, but is what is known as a core-implement; (b) it shows on its flaked surfaces—which are slightly glazed—a dense black colour,

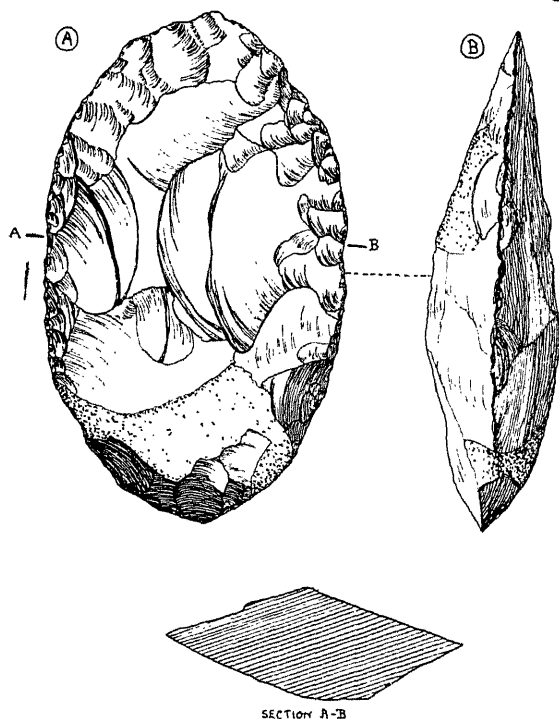


FIG. 1.—TWO VIEWS OF UNROLLED IMPLEMENT OF LOWER PALÆOLITHIC AGE FOUND ON THE BEACH AT ECCLES-ON-SEA, NORFOLK. $\frac{1}{2}$ nat. size.

interspersed with certain greyish patches; (c) it is entirely unpatinated, and unrolled, and carries neither incipient cones of percussion, nor striae, upon its flaked surfaces; (d) the primary flake-scars are large, while the secondary working along the edges is small, and is composed of many flake-scars caused by the detachment of resolved flakes; (e) in common with a number of Lower Palæolithic flint implements, it exhibits a heavily-truncated, "tranchet-like," flake-scar upon one side of the more pointed end; (f) both of its surfaces are flaked in a similar manner, and the cutting-edges are almost straight (Fig. 1B).

There can be little doubt that, judging from the form and flaking of this specimen, it must be referred either to the Late Chelles period, or to the succeeding epoch, viz., that of the Lower St. Acheul. Commont (*L'Anthro-*

pologie, Tome xix, 1908, Fig. 32, p. 348) figures a very similar implement, which he refers to Late Chelles time; but it must, in the present state of our knowledge, remain an open question whether certain specimens, such as that with which this note deals, are to be relegated to the latter phase, or to that of Early St. Acheul.

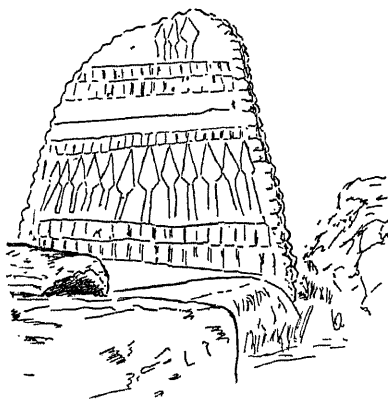
With a view of making myself familiar with the *provenance* of the implement found by Mr. Spelman, I recently paid a visit to Eccles, which lies just south of Happisburgh, and is situated on a very lonely and unfrequented part of the Norfolk coast. My examination of the sandy beach and low cliff exposed at this place failed to reveal any humanly-flaked flints, and I was impressed with the unlikelihood of finding such specimens at Eccles under the circumstances obtaining at the time of my visit. I concluded, therefore, that, from the quite unabraded condition of the Eccles palæolith, it must have been derived, possibly just prior to its discovery by Mr. Spelman, from some bed underlying the beach at this spot. I accordingly wrote him upon the matter, and his reply, in a letter, is as follows: "Your assumption is correct, as the time I found it there had been a 'scour' of the beach, and I found the implement in a bed of large flints, and picked it up simply on account of its shape, not in the least thinking of prehistoric finds." It is not a feasible proposition, which may, however, appeal to some ingenious minds, that this specimen was dropped by some wandering archæologist immediately before Mr. Spelman arrived upon the scene, and we may thus, with confidence, conclude that the implement is to be referred to a stratum present beneath the beach deposits. In the Geological Survey Memoir ("The Geology of the Country around Cromer," explanation of Sheet 68E, p. 87), dealing with the part of the country under dis-

Assam : Archæology.

Hutton.

A Carved Stone at Kigwema in the Naga Hills. By J. H. Hutton,
C.I.E., I.C.S.

44



In my paper on "The Use of Stone in the Naga Hills," read to the Royal Anthropological Institute on the 8th of April, 1924, I referred to the general absence of carving in stone in the Naga Hills, and mentioned such carved stones as had come to my notice. Since then I have seen an ancient carved stone in the Angami village of Kigwema. It is roughly incised with the "enemy tooth" pattern and with what must be spear heads, though they might almost pass as lotus buds. The edge of the stone is cut into double indentations, perhaps representing breasts. Anyhow they form a tally of reputed love affairs. The stone is so old that, although the name of the man whose memory, or whose soul, it embodies—one Honi—is known, it is no longer remembered to what kindred he belonged, and no one now claims descent from him. He had a love-charm of great potency, which made him irresistible to women, even when he became very old and bed-ridden. He used then to keep it in the bowl of his tobacco pipe; but if a woman picked the pipe up, she was at once fascinated by him and could not leave the house until he caused her to go. The stone is, therefore, clearly associated with fertility, and the carving, in particular the spear-heads, suggests some connection with Dimapur and Jamuguri (*vide J.R.A.I.*, Vols. LII and LIII).

J. H. HUTTON.

REVIEWS.

Anthropology, Physical.

Schlaginhaufen.

Die menschlichen Skeletreste aus der Steinzeit des Wauwilersees (Luzern) und ihre Stellung zu anderen anthropologischen Funden aus der Steinzeit, von

45

Dr. Otto Schlaginhaufen, O. Professor der Anthropologie und Direktor des Anthropologischen Institut der Universität Zurich. (Eugen Rentsch Verlag, Erlenbach, Zürich, München und Leipzig, 1925.) Pp. 278. 12 plates, 52 figs. in text.

Those who have gone by train from Luzern to Basel may remember that, when about 25 miles of the journey has been covered, the railway skirts the wide flat turf moor of Wauwil. The moor in reality is a filled up lake, and buried deeply in its turf have been found the remains of several pile-dwellings. From the objects found and from the fauna represented it is inferred that these dwellings were occupied in the earlier division of the Neolithic period. At various times between 1901 and 1924 and at seven sites, remains of the pile-dwellers have been found. At one site an imperfect left femur was found; at another merely a heel bone, but there were two of them which yielded finds of importance. At one of

these was found the complete skull and main parts of the skeleton of a remarkably small woman—a pygmy in stature. At the other was found the greater part of the skull of a man and a thigh bone, stout and curiously shaped, but it is possible that the skull and thigh bone may not be parts of one and the same individual. On these scattered remains of a few Neolithic lake-dwellers, particularly on the skull and skeleton of the little woman, Professor Otto Schlagenhaufen has trained the vast and complicated artillery at the disposal of the modern anthropologist. Indeed this monograph may be regarded as the most complete demonstration of what can be done by a thorough application of the technique of physical anthropology to the resuscitation of the living from dead human bones. And yet I have to confess that, after toiling my way through the hundreds of measurements, indices, angles, arcs, chords and sines, I turn gratefully to the simple, accurate and measured outline drawings of the little woman's skull and bones, for from them one can read so quickly and so intimately all her essential characters. Since the late Professor Kollmann made them known to us we have all been interested in those pygmy women which are so often found in the Neolithic graves and deposits of Switzerland. The example now described in such detail by Professor Schlagenhaufen is the smallest of the series. Her stature was 1.423 m. (4 feet 8 inches) and she was small in head, face, body and limb. Her cranial capacity was only 1,150 c.c., the length of her skull being 168 mm., its width 130 mm., its basi-bregmatic height 130 mm., and its auricular height 105 mm. The face was short—95 mm.—and wide in comparison to its length—124 mm. When the profile of her skull is fitted into that of the man's skull—from the same level of a neighbouring site—it is found—as Professor Schlagenhaufen has himself observed—to be, in every detail, a miniature of the man's skull. The man's skull overreaches that of the little woman at every point—its length is 190 mm., its width 144 mm., its auricular height 116 mm.; there cannot be a doubt, if cranial form is to guide us, that both man and woman were of the same race, but he was certainly no pygmy; such an identity in cranial form, I suspect, must signify a family relationship.

The cranial form of both man and woman is not unfamiliar to those who have made researches on human skulls dredged from the bed of the Thames, especially from the Mortlake reach of that river. On the banks of this reach foundations of pile-dwellings have been observed. The objects dredged from the sites of these dwellings suggest that they were built and inhabited in the Bronze Age. I do not think there can be a doubt as to the identity of certain crania dredged from the Thames and now in the Museum of the Royal College of Surgeons and the two crania figured here by Professor Schlagenhaufen. Others of the same type appear in the monograph by Studer and Bannwarth.

Professor Schlagenhaufen is inclined to trace a relationship between the racial type represented by the pygmy woman and the Grimaldi and other female types found in strata of Aurignacian and Magdalenian dates. For my part, I see a nearer relationship between the pygmy type and one which occurs in the cave at Offnet than with any other Palæolithic type. However this may be, I am convinced that physical anthropology can never explain such problems as that raised by the racial characters of the pygmy woman, until it has recognised that such problems are physiological in nature—they are problems of growth. Physical anthropology in its modern developments has lost sight of what it originally set out to measure. In the present instance we see from an examination of stature, head and face that growth in this individual has ceased at an adolescent stage; we are dealing, not with a pathological condition, but a physiological one which is well known to medical men. They are familiar with infantilism in its many pathological forms, but in the production of this pygmy woman we are witnessing a physiological

manifestation of this kind of growth. It is the same problem as the occurrence of dwarf races in breeds of domestic animals. Negro races, in particular, are prone to give rise to pygmy breeds, and there is a certain degree of resemblance in head and face form in all kinds of pygmies, no matter what race of mankind they may be produced from—just as there is a high degree of similarity among Europeans, Chinamen and Negroes who are the subjects of Achondroplasia. I would explain the miniature women who occur so frequently amongst the Neolithic inhabitants of Switzerland as manifestations of a certain physiological tendency which was inherent in that population. Whether such a suggestion meets with Professor Schläginhaufen's approval or not, I am certain that anthropologists owe him a deep debt of gratitude for his splendid monograph.

ARTHUR KEITH.

Genetics.

Hurst.

Experiments in Genetics. By C. C. Hurst. Cambridge University Press. 1925. Pp. 578. Price

46

Dr. Hurst's experiments in crossing began as early as 1894 and have continued since that time, with a break during the War, so that they include six years of the pre-Mendelian era as well as the whole period of the development of genetics. His numerous papers are here reprinted and form a valuable record of his work on inheritance in plants, animals and in man. Beginning with orchid hybrids, Major Hurst went on to rabbits, poultry, coat colour and other questions of breeding in horses, also pigeons, barberries, snapdragons and, finally, roses. In all of these fields he made contributions to the current problems.

But we are especially concerned with his work on inheritance in man. In 1907 he recognised the first Mendelian character in man, finding that blue eyes are inherited as a simple recessive to brown. Afterwards, hair colour, skin colour, complexion, left-handedness, musical temperament and other features were studied. He was among the earliest to take up observations on each of these characters. He made a preliminary analysis of complexion-colour and its inheritance. He also found, in accordance with Jordan, that left-handedness was a simple Mendelian recessive, and, further, that ambidexterity was a condition usually derived from inherited left-handedness.

R. R. G.

America, North: Technology.

Guthe.

Pueblo Pottery-making. By Carl E. Guthe. Yale University Press (Humphrey Milford). New Haven, 1925. Pp. ix + 89. 18s. 6d. net.

47

A detailed study of the processes of manufacture of hand-made pottery appeals to the archaeologist no less than to the ethnologist, especially when, as in the present instance, the ware is decorated with slips and paints. In the work under review the author gives a precise and painstaking account of the methods of the modern potters of the Pueblo of San Ildefonso, a Tewa village in New Mexico. The industry as it now exists is, it is true, a revival fostered by American enthusiasm, but it is a revival of methods which had not become entirely obsolete, and old or ancient examples of the native craft were used as standards of excellence. The modern wares may, therefore, be regarded as essentially Indian, and the technique as having suffered little, if any, contamination.

Dr. Guthe describes, and illustrates with excellent photographs, all the processes that lead up to the emergence of the finished ware—procuring and winnowing the clay, tempering, mixing, making slips and paints, shaping, slipping, polishing, painting, and firing. He also gives tables showing the time taken for each stage of the work. The modelling-spoons of gourd, the scrapers, the polishing stones, the paint-brushes (of frayed-out slips of Yucca leaves), the pads for applying slip, are described, and their uses explained. The process of shaping is that of coiling on

a moulded base. Slip (white, red, or orange-red) is then applied by means of a pad of cloth used as a wide brush; in one case (white slip) five or six coats are needed. The designs are painted on the slip, when this is dry, and for paints the orange-red slip is used, and also a black paint of vegetable origin; for decorating black polished ware a special clay-paint is used.

Firing is done with dung (horse, cow, sheep), in the form of cakes, and the process is very short—sometimes less than half-an-hour. Polished black ware is made by smothering the fire with fine loose manure, at the end of the firing. The author appears to consider that the black colour is due to deposition of carbon from the smoke (as is, of course, well known to be the case with the black polished pottery of the Baganda), but his account leaves room for the suspicion that it may really be due to the reduction of the iron in the slip to the black oxides. It is no reproach to the author to say that he is clearly not a chemist—he frequently speaks of “solutions,” or even “saturated solutions,” of the slip-clays—but it is a defect in the book that there is nowhere any attempt to describe, even approximately, the composition of the potters’ raw materials. There would appear to be, indeed, a scrupulous avoidance of any suggestion that chemistry could come into the picture, and in so far as this has led to the absence of guesses, it may be commended. But chemists, like most of us, may be hired, for love or money.

The introduction, by Dr. Kidder, gives a very interesting account of some of the conclusions arising out of recent work in the South-West, and of the chronological results that are emerging.

The illustrations are numerous (35 plates and a few text figures) and form not the least valuable feature of the book. The whole work reflects great credit on the author, on the Director of the Pecos Expedition (Dr. Kidder), and on the Department of Archaeology of the Phillip’s Academy, Andover (Mass.) But one would have liked a few analyses, to complete a record of such high value.

H. S. H.

Evolution.

Sonntag.

The Morphology and Evolution of the Apes and Man. By Charles F. Sonntag, M.D., Ch.B., F.Z.S., with a foreword by Professor G. Elliot Smith, M.D., F.R.S. London: John Bale, Sons & Danielsson, Ltd., 1924. pp. 356. Price 12s. 6d. **48**

Dr. Sonntag’s book is the result of much laborious work, carried out by him at the Prosectorium of the Zoological Society of London. It is essentially a collection of data concerning the gross anatomy of the anthropoid apes, and is valuable not only for the mass of anatomical information which it contains and which derives its source largely from the author’s own observations, but also for the extensive bibliography, which consists of over 500 references. The main part of the book, which deals with the Simiidae, is preceded by short accounts of the anatomy of the Lemuroidea and Tarsius and the Old and New World monkeys, which serve to indicate the position of the anthropoids in relation to the lower primates. No attempt is made to detail the relations which the primates bear to other classes of mammalia. The last sixteen pages of the book form a chapter on the evolution of the Primates. This mentions briefly the most important fossil primates which provide evidence bearing on this subject and contains a synopsis of the current views regarding the genealogy of the apes and man. We are somewhat disappointed not to find here an analysis of the anatomical data, recorded in the rest of the book, in support of the interesting genealogical tree shown on page 319. In this diagram, the differentiation of Man and the African anthropoids from a common *Dryopithecus-Sivapithecus* group is indicated in the middle Miocene period. The importance of this group of fossil Primates in connection with the origin of Man is rightly insisted upon, and we take the opportunity of noting this here because we believe that the evidence.

in favour of Pilgrim's claim for *Sivapithecus* is stronger than most palæontologists appear to think.

As will be gathered from this brief description, this work will be of use primarily to students of anatomy and anthropology as a text-book and as a book of reference. It necessarily presupposes a knowledge of human anatomy. It contains information which is essential to anyone who wishes to understand and to gain a true perspective of the relation between Man and the other members of the Primates. for, as Professor Elliot Smith points out in his foreword, it consists of a statement of facts which is wholly unbiassed. Dr. Sonntag is to be greatly thanked for this extremely useful record.

W. E. LE GROS CLARK.

CORRESPONDENCE.

Archæology.

Barnes: Moir.

To the Editor of MAN.

A Misleading Exhibit.

SIR,—Mr. S. H. Warren, under the auspices of the Royal Anthropological Institute, exhibited, during the month of February, a number of models of flint-flaking, in a further attempt to support his erroneous views on this question. Mr. Warren showed certain models which he claimed, incorrectly, are representative of the large series of flaked flints from the pre-Red Crag, and the Cromer Forest Bed horizons, and has, in addition, conceived the notion that it is possible to differentiate between flint flakes struck by human agency, or produced by pressure applied by man, and flakes formed under non-human, natural conditions, by pressure or concussion, by using, as criteria for this differentiation (a) the size and position of the *écaillage*, and (b) the shape of the bulb of percussion. For example, Mr. Warren exhibited a model intended to illustrate what he terms a "normal" flake, struck by human agency, which is marked A². This specimen (A²) is stated to be representative of a flake produced by "muscular blow normal," and we propose to select this assertion as an illustration of Mr. Warren's unscientific methods. Apart from patination, secondary work, and other general features, such as size, etc., the only way in which one simple flake differs from another is in the character of the striking platform and bulbar surface, and the size, shape, and position of the *écaillage*. In Mr. Warren's model A² the *écaillage* is shown on the right hand side of the longitudinal axis of the flake—of a certain size—and in a certain position relative to the point of impact, while the flake itself has a bulbar swelling of a certain contour. These characteristics are inherently variable, and outside human control, and no fixed configuration or arrangement of them, such as Mr. Warren's models exhibit, can be considered as representative of a humanly-struck flake. For instance, out of 330 bulbed flakes of palæolithic age which we have examined, the *écaillage* lay on the left-hand side of the longitudinal axis of the flake in 70 cases, on the right hand in 80 cases, and on the axis itself in 31 cases, while in 149 cases the *écaillage* was absent. Further, its position on the bulbar surface varied widely, and its shape varied from a length of 3 mm. to 90 mm. The contour of the bulbar swelling of the flakes varied with equal freedom, and it is manifestly impossible to construct from such characters, normal, or representative, flakes characteristic of the results of human blows, or pressure, or of natural concussions or pressure.

Mr. Warren's models, therefore, can only be treated as isolated examples, and not in any way representative of a class. In the present state of our knowledge it is not possible to discriminate between the special characters of certain flakes from the Sub-Crag horizon, the Cromer culture, and those of well-known palæolithic deposits. In his notes accompanying the models, Mr. Warren appears, perhaps not very clearly to have recognised this variability of the characters exhibited by flint flakes, without, however, realising that this fact deprives the whole of his exhibit of its *raison d'être*.

It will be clear, then, that the claims made by Mr. Warren, by means of these models, have no basis in fact; and because of this, and the suggestion (MAN, 1926, 24) that duplicate sets of these models should eventually find their way into museums, where they would be consulted by those anxious to acquire an accurate knowledge of the fracture of flint and of early implemental forms, we wish to state, for the reasons given in this note, that this exhibit is misleading.

Yours faithfully,

ALFRED S. BARNES,
J. REID MOIR.

Religion.

Thomas.

To the Editor of MAN.

The Use of Sand in Magic.

SIR,—Having read Professor Canney's interesting article on "The Use of Sand in Magic and Religion" (MAN, 1926, 6, 18), I was prompted to turn to the "Golden Bough," where, as the writer of the article no doubt also found, there is only a solitary reference to sand, and that not relevant to the subject in hand. 50

I think, if we consider the physical and natural properties of sand, we are rather given pause before accepting all Professor Canney's conclusions as to magical properties attributed to it. Sand is cleansing (bodies are preserved when buried in sand in dry climates), and this would be as obvious as the cleansing properties of water to, and readily discoverable by, any people acquainted with dry sand in quantity. Sand is also clean and very tractable, and can be used like water (poured out, *i.e.*): it is admirably suitable for making a nice, clean, flat dry surface for a grave or for a road in a festival. Is it surprising that it should be quaintly used to write mottoes or verses in places where married couples are so felicitated and sand plentiful? The original of *this* custom, and *not* the use of sand it seems to me, is of chief interest in connection with Knutsford.

That it should have been used as a base to prop up mummy coffins and statues in Egypt is perfectly comprehensible to any one who knows the ancient tomb districts of Egypt; it is also ready to hand, with water, on the sandy river banks or sea shore in India for moulding emblems of the gods. Fortune-telling in the sand by desert peoples, again, is readily accounted for: it is there to draw figures upon. Similarly as to its ceremonial use by Moslems instead of water. It seems, perhaps, then that the chief interest of the article centres in its use in large quantities in barrow burials in Britain and tumuli in Scandinavia, and that not, may I venture to suggest, in connection with any beliefs in mystical properties of sand, but because the custom seems to point to an origin in a country where sand is plentiful?

I am, etc.,

Pitt Rivers Museum,
Oxford.

E. S. THOMAS.

To the Editor of MAN.

SIR,—A recent discovery may be of interest in connection with Professor Canney's notes (1926, 6). In the centre of a Round Barrow in Ysceifiog Parish, Flintshire, I found a cairn of stones; this cairn covered a filled-in grave pit, on the floor of which lay the disintegrated skeleton of a man. The pit had been dug through 5 feet of surface soil and gravel, down to a horizontal layer of fine white sand. On this sand the dead man had been laid. No grave goods were present, but an upper limit of date was fixed by the presence in the cairn of a secondary burial by cremation associated with an overhanging-rim urn. The

primary burial thus belonged to an inhumation period prior to the cremation phase of the Bronze Age; that is, it was either of the Early Bronze or Late Neolithic periods.

At the time of the discovery (July, 1925), I had no doubt whatever that the position of the layer of sand determined the depth of the grave pit.

A report on this burial will be published in *Arch. Camb.*, 1926.

CYRIL FOX.

Africa, West: Technology.

Philipps.

To the Editor of MAN.

African Throwing Knives.

SIR,—Owing to absence from Western Europe and almost continuous travel since the end of June last, volume LV. of the JOURNAL has only just come into my hands, and with it the most useful and enlightening paper by Mr. Thomas. 51

The *Gonyo* throwing-knife, so called from its similarity to the bird of that name, is now, I think, practically extinct among the Zande peoples. Any survivals of it would more probably be found north-west of the Mbomu in the (French) territory into which older Azande believed their forebears to have first entered from the north-west, than anywhere else. It is there that the first settled and least modernised Azande are to be found.

The Abandiya of the Bas-Uele have a narrow knife of about a foot long, apparently simplified in modern times, of which the last two inches or so are curved. It is alleged to have been, in the first years of Vongara domination, used for decapitations.

It is known as *Mbungö* and is sometimes stated by the people (I think doubtfully) to have been in origin a throwing-knife.

Such knives are now more in evidence among the Baza ruling caste than among the mass of the Abandiya, who have been a unit of the Zande confederation and are almost entirely Zande-ised.

The Kpiyinga (or *Kp̄̄nga*), which is becoming of less practical use in these latter days of suppression of tribal warfare, is probably derived from the root *kpi*: die (*kpiyo*, or *kpiyē*: death). Whence perhaps the compound:—

kpiy (e)-nga-he = *kp̄̄nga*(h),
death - is-it, the lethal weapon.

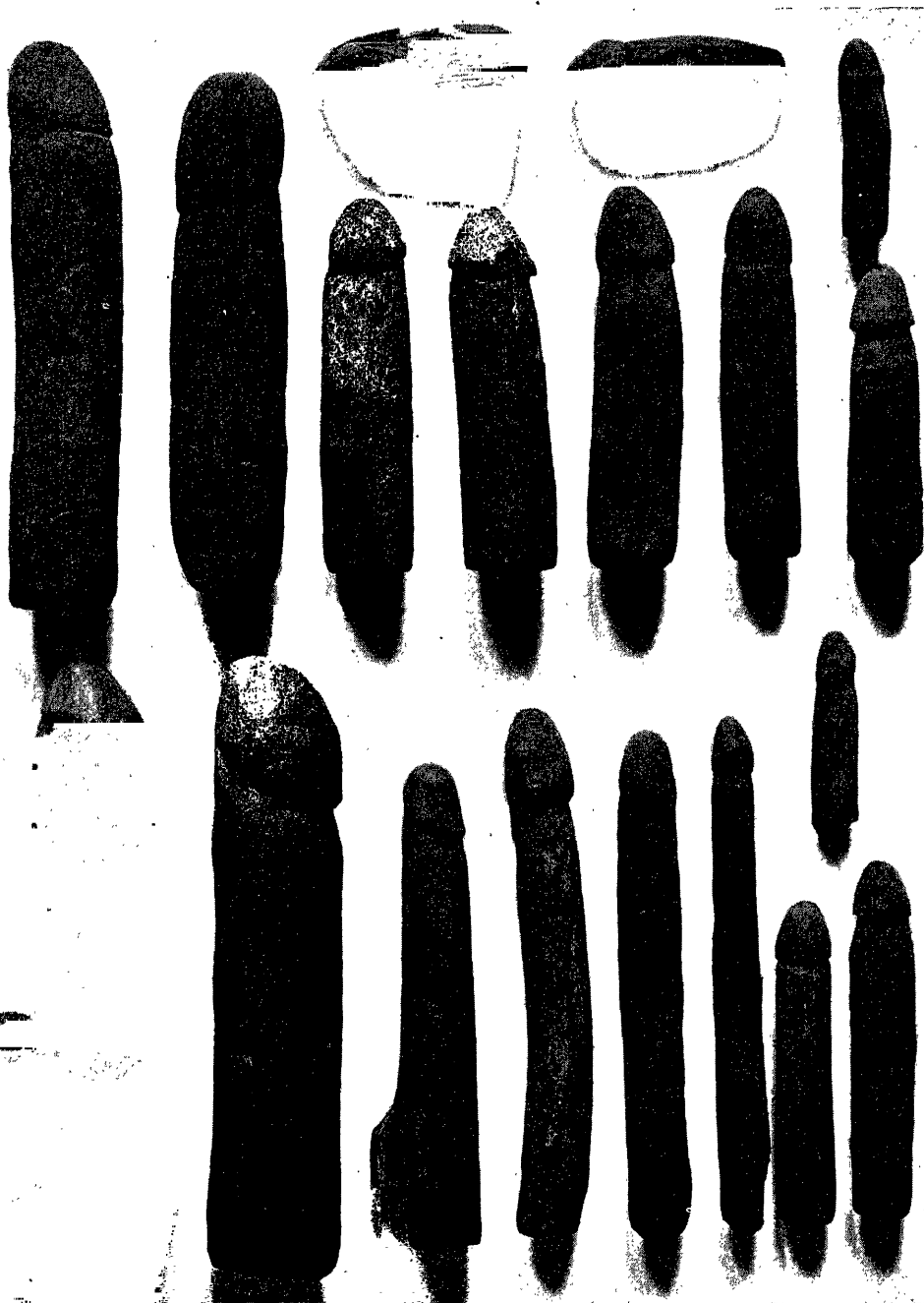
Types PX2 and PX3 are frequent. Some *akp̄̄nga* of the Hima* Azande and a squarish bill-hook type of knife† of an old pattern, given me by Sultan Ekibondo of the Bangba-Mangbettu, contain, in the thickest part of the metal parallel to the handle, two round holes. Perhaps these have been retained in the gradual solidifying and squaring-up of the irregular contours of the primitive type to meet changing functions.

The two holes are so spaced as to permit of them being used as peep-holes. In the presence of certain chiefs of the ruling caste, messengers or suppliants of the subject-race were expected to place and retain the knife close to the upper part of the face. The looking through the eye-holes appears to have served the double purpose of veiling the eyes in acknowledgment of the radiance of the "presence" and of showing the weapon "above the board" before approaching and while craving the boon.

TRACY PHILIPPS.

* Hima.—The BaHima are a Zandeised but still bilingual Bantu tribe of the French and Sudan territories. Beyond the group-connection, their language has a disappointing lack of detailed resemblance with that of the BaHima of Nkole or of the BaTutsi of Ruanda.

† Sometimes known colloquially to aliens as *Zobia-zobia*.



PHALLIC OFFERINGS TO HAT-HOR.

ORIGINAL ARTICLES.

With Plate E.

Egypt: Archæology.

Hornblower.

Phallic Offerings to Hat-hor. *By G. D. Hornblower. With Plate E.*

52

Dr. Naville, in "The XIth Dynasty Temple of Deir el Bahari," I, 65, col. 1, records the finding of some wooden phalli in the 18th Dynasty shrine of Hat-hor which held the beautiful cow-statue now in the Cairo Museum.

This find seems to have attracted little, if any, of the attention which it surely merits, and so, on learning from Mr. C. T. Currelly, Director of the Royal Ontario Museum of Archæology, Toronto, that the objects were deposited in that Museum, I asked him if he could supply any information about them and am much indebted to him for the favour of the photograph reproduced in Plate E, and for the following note, which is the more valuable that he was working with Dr. Naville at this site and was present at the opening of the shrine.

He writes that the shrine "was buried in débris and was revealed to us suddenly by a man pulling out a stone underneath and a huge fall coming, which revealed something like three feet or more from the roof. So the phalli were lying about on the floor, many of them not covered up by the rubbish, and a few revealed after the rubbish was removed. The back of the shrine had been restored, presumably by Seti I., and the shrine must have been abandoned immediately afterwards, as the paint was too fresh to have been more than a comparatively few years old—I should say not more than twenty years. The rubbish was made up of cuttings, broken pieces of stelæ, odd bits of inscriptions, dust, stone chips, everything that is found when a temple is rehewn and the hewn stones carried away for another building."

He adds that they were all of wood and devoid of any inscription or marks of any kind. "There was no order; they had simply been dropped around. There was practically no dust in the place, except where it came down from the front; I could trace no order whatever in the laying out of these objects."

This very interesting note makes it clear that the phalli were found as originally placed, and we have here an example of an ancient Egyptian practice of which no other such definite record exists.

There can be little doubt that these offerings point to the popular belief in Hat-hor as a fertility goddess and are symbols of prayers for the bearing of children, with perhaps a direct aphrodisiac intention, made and sold by the priests to poor devotees. (Compare the account of the late "Bes Chambers" of Saqqara, below.) They cannot be *ex-voto* offerings for cures, as in ancient Greek temples (*v. Rouse, "Greek Votive Offerings,"* 210, n. 8), for then models of other parts of the human body would have been found; nor are traces of such offerings found elsewhere in ancient Egyptian remains.

Parallel offerings are found in the baked clay figurines of nude broad-hipped women found at the 18th Dynasty Hat-hor shrine of Faras, Nubia (*v. Griffith,*

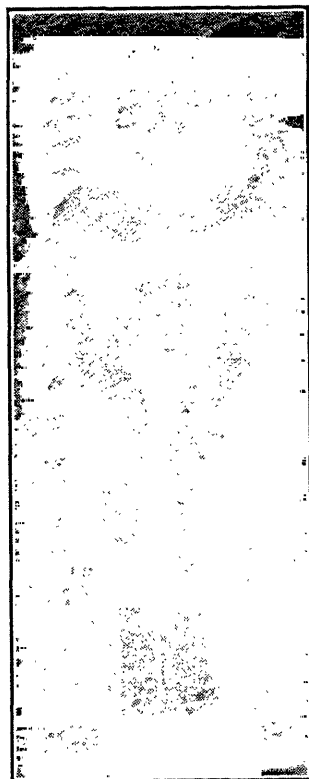


FIG. 1.—RED POTTERY FIGURE OF NUDE WOMAN ON A BED. SIZE 6 IN. × 2½ IN.

"Oxford Excavations in Nubia," *Liv. Ann. Archæol.*, VIII, p. 87) and at the Hat-hor shrine of Deir el Bahari of the same period (v. "The XIth Dynasty Temple of Deir el Bahari," III, Pls. XXIV, 2, and XXXII, 8 and 9). These might, perhaps, be *ex-voto* offerings for children born, but, in view of the phalli, are more probably, like them, symbols of prayers for the bearing of children, chosen, of course, on the well-known principle of sympathetic magic.

The ear is another part of the human body of which models figure in offerings to shrines. Such models, in glazed faience (they are also found in wood), were found at both the Hat-hor shrines above mentioned (v. Griffith and H. R. Hall, *op. cit.*) and also at Memphis (v. Petrie, "Memphis I" p. 7 and Pls. IX to XIII), where many tablets were excavated bearing images of ears, some in large numbers—110 in one example. Their meaning is definitely settled by the prayers to Ptah (chief god of Memphis) inscribed on them, calling on him to listen; there can be no question of *ex-votos* for cures. Such offerings might be made at any shrine, one to Amon-rè is given in Wilkinson, "The Ancient Egyptians," II, p. 358.

Professor Sir Flinders Petrie states (v. Griffith, *loc. cit.*) that no examples of the female figurines or of ears were found at the Hat-hor shrine of Serabit el Khadim, Sinai, but the nude woman's figure shown by him in "Researches in Sinai," fig. 151, no. 14, lying on what is probably a bed, is undoubtedly an offering of the kind under review, for most of the figures at Faras are of this type (v. Griffith, *op. cit.*, Pl. XIX). Similar ones are frequently brought to light by dealers; I have one*, of coarse red pottery, in which the wig is of the Old Empire type, probably of Saitic times or a little later; the backing represents clearly a bed woven of rushes (Fig. 1). Occasionally a child is

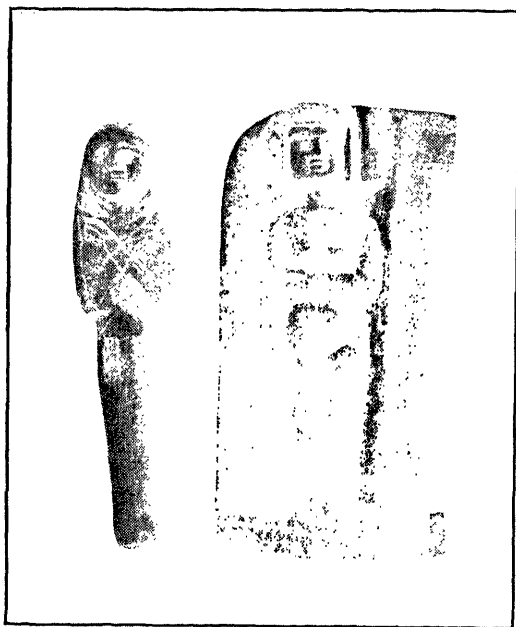


FIG. 2.—NUDE WOMAN AND USHABTI FIGURE
R.O.M.A., TORONTO. LENGTH, 5½ IN.

added by the woman's side, as in an example found in the 18th Dynasty cemetery of Gurob (v. Petrie, "Ehnasya," Pl. XL, No. 20 and p. 25); this type is said by Petrie to have been common at Naucratis (v. "Naucratis," I, p. 40); the examples illustrated by him (*op. cit.*, Pl. XIX) show the bed, but none have the child. They appear to have been found among the house-rubbish, though it is difficult to understand why they should be kept in houses if they were *ex-voto* offerings or funerary figures. That they were probably of funerary use is shown by a set of five figures in white limestone, with black painting, obtained from a Cairo dealer, and now in the Royal Ontario Museum of Archæology (Fig. 2): one represents a nude woman couched, as in the examples from Naucratis; three are *ushabti*, and one is a *reis-ushabti*. They are probably of the Saitic period and provide, perhaps, a fresh example of the well-known archaizing tendency of that period, for figures of nude women are found in tombs from the earliest times down to the 18th Dynasty. It is possible that the examples found in the house-rubbish at Naucratis came from a factory of such figures; they were intended

* Now in the British Museum.

for funerary use only,* and show an ignorant confusion between early funerary figures of nude women and later *ushabti* figures.

It may be noted that phalli in stone or baked clay are sometimes seen in the hands of country dealers in Egypt, with no real indication of their place of origin, and also various obscene objects in glazed faience, all of late date, mostly Græco-Roman†. Of these it is interesting to note an example seen by me illustrating the incident of a woman and an ass *in coitu*, related by Apuleius in "The Golden Ass" (v. p. 209 of Bohn's translation), for it shows that the incident was a current theme in Egypt centuries before Apuleius wrote; perhaps he had seen some such figure in Egypt or, more probably, heard a story which he embodied in his collection of strange tales and adventures.

The openly licentious tendency of these later times of religious confusion is well illustrated by the "Bes Chambers" found at Saqqara (v. J. E. Quibell, *Excavations at Saqqara*, I, 12-14) where painted figures of Bes, furnished with great phalli and holding a figure of a naked woman at each arm, were fixed to one wall and figures of naked women stood opposite, fixed to the other, while stone phalli and obscene statuettes strewed the ground in all stages of manufacture, showing that they were made on the spot, as has been suggested for the objects found in the Hat-hor shrines. These chambers could hardly have been shrines for worship, but were probably, as Quibell seems to suggest, centres for the practice of aphrodisiac magic connected with Bes.

G. D. HORNBLOWER.

Italy: Archæology.

Crowfoot

Note on Excavations in a Ligurian Cave—1907-09. By Mrs. J. W. **53**
Crowfoot.

The cave known as Grotte de Bertrand or Tana Bertrand was explored, as so many others have been, by the entomologists MM. Doderò and Spagnolo, and is consequently listed in P. Bensa's *Guide to Ligurian Caves*.‡ He gives the following description of it (translated):—

"*Taggia Valley*. Tana Bertrand. Near the summit of Monte Fando (Badalucco). The entrance opens in a cleft in the eocene limestone, and is "difficult to get to owing to the want of a path. The beetles *Anophthalmus* "Spagnoli and *Bathyscia* Spagnoli are found there."

This description aroused my interest, and, being at San Remo in 1906, I went, with friends, to try and find Tana Bertrand—and the blind beetles. The cave seemed well known to the people of Badalucco, and a source of lively interest to them, although it is of no great extent, and has no stalactites or other striking natural beauty. "As old as Tana Bertrand" is a favourite saying among them of any object of surprising antiquity. It is supposed to be haunted—at night strange lights float round the entrance—poor ghosts revisiting the scene of their pitiful death. "For," say the village folk, "long ago '*nei tempi antichi*,' in the "days of a great war, the inhabitants of Badalucco took refuge in the cave, and "the conquerors, having walled up the entrance, those within perished of hunger." This story, one often told of caves, has a historical foundation in Liguria (as probably elsewhere). Issel quotes classical authorities to this effect in his *Liguria Preistorica*: "The Ligurians during the first Punic wars often took refuge in "forests and caves . . . 'Fulvius, therefore, with great sagacity, found their "caves and hiding holes, and having closed the entrance, with a fire, burnt or "suffocated them, and so conquered them.'"§

* Of the origin and development of this funerary custom much might be said, but this is hardly the place for it.

† For the dating, *cp.* Petrie, "Amulets" *Phallics*, p. 11, par. 16.

‡ P. Bensa, *Le Grotte delle Appennine, Ligurie*. Club Alpino Italiano.

§ Sallust, *J. Cæsar*.

"And it must be true, that story of the cave," one informant said to me, "because one still sees there the remains of the victims and part of the wall that entombed them." The "wall" really only consists of pieces of stalagmitic concretions near the cave mouth, and the remains are prehistoric, probably of very late Neolithic or Early Bronze Age, but still anterior to that bitter struggle between their barbarian ancestors and the power of Rome which lives so strangely in the village memory. My first discovery was made before I heard these stories about human remains—I came across two jaw-bones and a humerus lying practically on the surface of the cave floor while I was hunting for the beetles. These bones were shown to Dr. H. O. Forbes, who considered them certainly neolithic, and afterwards to Professor Issel (Genoa), who confirmed this.

This promised rewards to further research, and consequently, in 1908, I went several times to the cave, and between then and May, 1909, worked there for 31 days in all. The "day" up there was not a long one—the journey from San Remo to Badalucco, accomplished by the local omnibus, took two hours, and the climb up from Badalucco to the cave about one and a half hours.

I obtained permission to excavate, through the recommendations of the parish priest, the Reverendo Domenico Orengo, who was enthusiastic about the discovery in the cave, and helped me in every possible way, as did his sister, Signorina Orengo. I often stayed with these kind friends at the Casa Parochiale, Badalucco, and so gained more time on the mountain side.

The position of the cave, on a rock face that would have seemed precipitous if it had not been so thickly clothed with ilex and other bushes, with a bottle neck entrance opening out on to a narrow platform, made work very slow. According to village talk, there should have been two vast halls, and a passage therefrom penetrating the whole mass of Monte Fando, and having an outlet in another valley near Dolcedo, proof being afforded by a tale of a dog—that unfortunate dog who runs through the underground passages of the world—going in at one end and coming out at the other, but minus his hair.* The actual dimensions are as

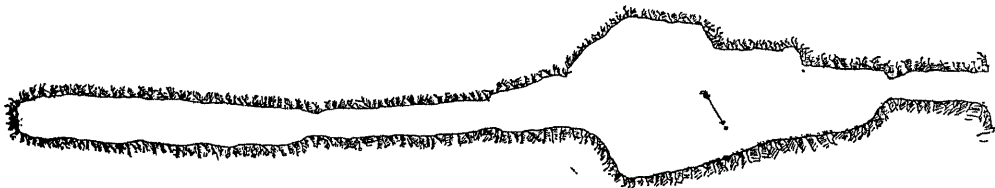


FIG. 1.—PLAN OF TANA BERTRAND.

follows:—Entrance, 8 feet 4 inches long, with a bottle neck 1 foot 7 inches wide and 1 foot 8 inches high; cave proper, 33 feet long by about 12 feet, passage leading from it 48 feet long ending in a hole like that of a fox or badger. The cave is 4 feet high in the centre, lower everywhere else. It faces west, and the daylight only enters effectively in the late afternoon. The method of working followed was to examine the earth *in situ* by artificial light, and then carry it out, in baskets, and sieve it on the platform. My usual assistant was Antonio Bianchi, an intelligent boy from the village; often friends came over from San Remo and helped also in the work; more than 40 persons, including the Rev. Orengo, visited the cave in the winters of '08 and '09. By May, 1909, fallen rocks had been removed from the cave floor, and the soil excavated to a depth of about 80 c. The remains from so small a section may be taken to belong probably to the same period. They include human and animal remains, worked flints (two only), bone pendants, bone points and beads. No trace of pottery or metal.

* Cf. Cresswell, Wooley Hole, Cheddar. Sometimes a cat, or, as at Lincoln, a duck that loses its feathers.

OBJECTS FOUND.

A. *Worked flints*.—Only two were found, both in a pale grey “cherty” flint. No such flint is known in the district. They are :—

Fig. 2, 3.—A lunate, or “bird” arrow-point (*armature de flèche à tranchant*). In shape resembles the Tardenoisian lunates, but is larger and rather coarsely worked. Such a form is not known from dolmens having the bone points (*poingons*) and the beads described below.

Fig. 2, 4.—Arrow or javelin head, thick and roughly worked, with a stem, but no barbs. M. Cartailhac wrote to me that he had flints similar to it. The flint most like it that I have seen is in the collection made by Dr. Sturge. said to come from Lake Trasimene.

B. *Worked Bones*.—One fine needle-like pendant, probably worked from a boar’s tusk (*Fig. 2, 1*). Six bone pendants similar to *Fig. 2, 5* and *6*, and also probably cut from tusks. Five bone points resembling *Fig. 2, 7*.

C. *Animal Remains*.—Very few were found. Besides the ornaments mentioned above, only a few bones of sheep, the rib of a pig (?), a dog’s tooth, and another of fox (?), and the tibia of a small ox, determined by Professor C. Andrews.

Numbers of shells of *Helix nicaensis*, ancient and recent, were found; this snail is very rare on that side of the Roia valley.

D. *Human remains*.—These indicated at least 10 individuals, calculated on 10 right heel bones. No complete skeleton was found, and only a few fragments of crania. Professor Issel, on an examination of them (May, 1909) said that “they presented nothing to distinguish them from the Ligurian race.” Certain of the remains show primitive characteristics, e.g., large teeth, heavy os calcis, abnormal shape of coronoid apophysis of jaw, perforation of the humerus, platynemia of the tibia, deep grooves for muscle attachments—others show nothing remarkable. The teeth are well preserved and ground very flat.

All the bones are now in the Bicknell Museum at Bordighera, with the exception of two fragments of cranium in the Museum at Genoa.

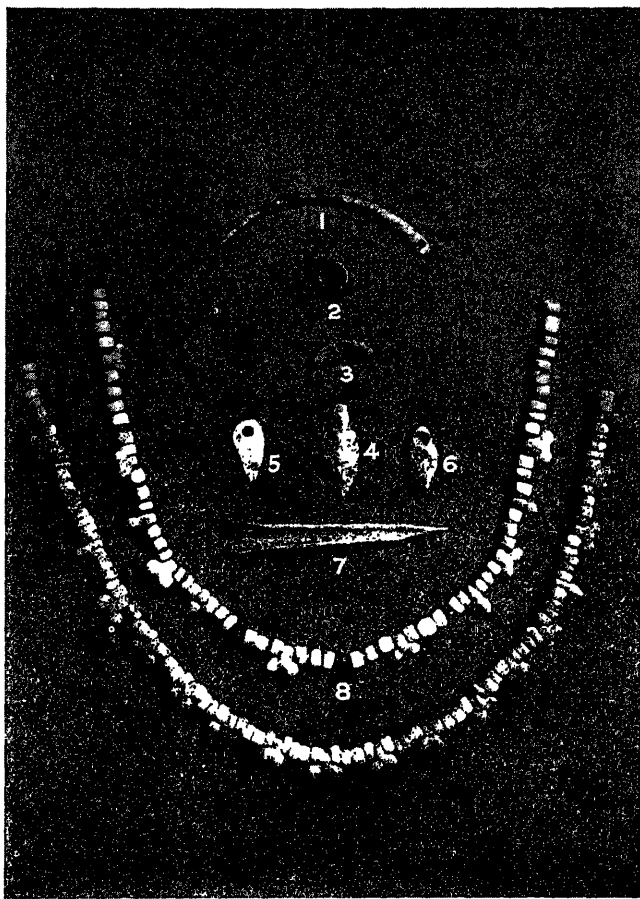


FIG. 2.—OBJECTS FOUND AT TANA BERTRAND.

E. *Beads*.—300 in all were found (some broken). The largest (Fig. 2, 2) is of hæmatite. Monte Negro is a likely *provenance* for this substance. The others are of five kinds:—

1. Winged beads (*perles à ailettes*) in calcite. The material might come from bands in the rocks of the district.
2. Single drop beads, also in calcite.
3. Flat round beads in same material.
4. Round beads in hæmatite.
5. Tiny flat round beads in what is probably a dark grey clay slate. There is a quarry of a good slate (*lavagna*) close to Trionra, and in the Middle Ages, and later, ornamental door lintels were made from it in all the villages round.

One dentalium shell was found, probably used as a bead.

In the upper necklace in the photograph (Fig. 2, 8) the beads are strung with the grey and white alternating. This is not merely a capricious arrangement; in several cases the grey beads were found stuck in the grooved hole of the white ones.

These necklaces are identical with some in the Museum at Toulouse—the shapes of the beads and material are similar—the same placing together of winged and round flat white beads in calcite, with small round flat beads in slate and hæmatite.

Those at Toulouse come from many places in the Ardèche, Gard, Hérault, Lozère, Aveyron, and Tarn-et-Garonne, for the most part found in ossuaries of dolmens, with objects of the Bronze Age. Similar winged beads were also found in the Grotte de Durfort (Gard).

There are more beads of the same types, both winged and round, in calcite, and various shapes in hæmatite in the museum at Geneva, also from the Cevennes. With them is also a dentalium shell.

A similar necklace is in the British Museum, in white beads only, from La Couvertoriade, Aveyron, given by M. Cartailhac.

These beads are described by Mortillet in *La France préhistorique*, p. 773: “*Perles à ailettes* . . . Il y a de ceux qui ont cru voir dans la perle à ailettes “quelque signification phallique, mais cette idée n’a pas de fondement.” Another necklace is figured by Cartailhac in *Les Âges préhistoriques de l’Espagne et de la France* with objects of the Bronze Age. M. Cartailhac mentions, in a letter to me, a winged bead and a *pendeloque* similar to mine (Fig. 2, 1), to be seen in the Musée de l’Histoire Naturelle at Marseilles, from the “Grotte sépulchrale de St. Clar,” but I failed to locate it there. The type is unknown in the Nile valley. Gordon Childe,* who gives a map of Europe showing places where these winged beads have been found, returns to the phallic theory of their form, and considers a bead from Paros (Fig. 20, 3) to be “the prototype of a series diffused from South France to the Baltic and the Don.” The series, to my mind, are more like each other than like the bead from Paros, and the variety of shape among them too is against the phallic theory, otherwise it would be tempting to consider the winged beads as symbolising the male principle and the single drop the female. One can sometimes ask too much meaning of what was very likely ornament pure and simple. For these winged shapes do please the eye, and, strung with the alternate dark and white round ones, make a very pretty little necklace.

CONCLUSIONS.

The small size of the cave, absence of hearths (except for one small place showing a thin layer of charcoal), the minute number of animal bones and absence of pottery, point to its not having been used as a dwelling place. It was certainly

* *The Dawn of European Civilisation*. 1925. pp. 44-45.

a sepulchre, probably a family vault. The position and condition of the bones indicate successive burials, and the use of an "ossuary." Nearly all the long bones and the portions of skull were found poked under the rock at one side of the cave and piled one on another in such a way as to make it impossible that they could be in their original position at burial. All the smaller bones and the lower jaws were found down the centre of the cave, as also were the beads and other small objects. I think, therefore, that the bodies were laid in the centre of the cave, and at each fresh burial some of the bones of the previous one were removed and placed on one side. An interesting parallel is to be found in the Grotte de Durfort (Gard).^{*} In that cave remains indicating about 50 individuals were found, together with worked flints, beads (winged and other), pendants and buttons in calcite or alabaster, and a blackish grey soft stone (*pierre ollaire*?), and 30 beads in copper. In the top layers the skeletons were found "assez sensiblement dans leurs connexions naturelles ;" in the lower layers "les ossements ne se montrent que dispersés et brisés."

The writer, Cazalis de Fondouce, concludes that "Il est donc probable qu'ici, comme à Orrouy (Oise) les derniers corps ensevelis étaient placés dans la couche superficielle, et qu'à mesure qu'on y portait de nouveaux cadavres, les ossements des premiers étaient déplacés et entassés dans l'ossuaire pour faire place aux nouveaux venus" (p. 260). Here, more fortunately than at Tana Bertrand, complete crania were found, said in this note to belong to "une race métisse Celto-Ligure." I do not know what more recent study has been made of them. As to the date of the Grotte de Durfort, Cazalis considers it to be "à la fin de l'âge de la pierre polie, ou, pour être plus exacts, de l'époque de transition entre cet âge et celui du bronze époque que nous appellerions volontiers l'âge du cuivre, si, au lieu de ne trouver que quelques perles de ce métal, on venait à rencontrer des armes ou des outils. Elle est contemporaine de S. Jean d'Alcas, dans l'Aveyron, de l'époque des dernières constructions mégalithiques." It looks very much as if Tana Bertrand was used in the same period too, whatever you call it.

Professor Issel, after seeing all the remains found, considered that they were late neolithic, and M. Cartailhac took a similar view on specimens shown him.

On the general question of Ligurian caves, Gordon Childe says: "I must repeat that nearly all the neolithic caves have yielded also copper objects or flint of chalcolithic type, and neolithic types recur in the copper age dolmens of South France. So the civilisation just described may not be really 'neolithic' at all, but just a survival due to a barbarous people living in a backwater during the copper age and the bronze age."[†]

M. Cartailhac touched on the same problem in a letter to me in August, 1908. Speaking of the find at Tana Bertrand, he said: "Votre trouvaille est vraiment précieuse, elle lie les Causses des Cévennes au nord italien de telle manière que nous en aurons, je pense, des clartés inattendus . . . Il (Issel) a raison de placer vos objets à la fin du néolithique. Tous les mobiliers funéraires des dolmens de notre Midi sont fortement pénétrés de cuivre et de bronze. Si bien que je ne sais plus si le Midi de la France et l'Italie ont un néolithique vraiment pur de tout mélange. Au dessus de la Dordogne dans tout l'ouest et nord de la France il y a le niveau que représentent si bien en Scandinavie les *kjokkenmoedings* et qu'on a appelé Campignien. Ce niveau A, franchement néolithique, manque autour de la Méditerranée, où il est remplacé sur une très large bande de pays (la Suisse comprise) par le néolithique B., peu susceptible de divisions

^{*} *Matériaux pour l'Histoire de l'Homme*. 2 Série. No. 5, Mai, 1869, p. 260.

[†] *Op. cit.*, p. 262.

“ chronologiques et qui nous réserve encore bien des surprises de l’Espagne à l’Asie J’espère bien que vous pourrez réaliser votre projet “ et fouiller à fond la grotte de Badalucco.” That hope has not yet been realised, and there is no doubt that more remains to be found there. My excavations did little more than scratch the surface. Still, it seems better to publish a fragmentary record than to wait any longer for a fuller one. I must end this note with a word of homage and gratitude to the memory of Issel and Cartailhac, to whose counsels and encouragement anything of value in it is due. GRACE M. CROWFOOT.

Malay Peninsula: Technology.

Schebesta.


The Bow and Arrow of the Semang. *By the Rev. P. R. Schebesta.*

54

In his work “ Pagan Races ” (London, 1906, Vol. I, p. 277, ff.) W. Skeat gives a long discussion of the bow and arrow of the Semang, in which he points out several anomalies which have not yet been solved. Concerning them, I can, from my own investigations, say the following:—

The bow is made from two kinds of wood, either of *jehu baros*, a black kind of wood, or from *langset*. The latter is brighter. I have had bows made in my presence and have also been in the forest with the natives to find suitable material for them. We found a little *langset* tree which they pointed out as very suitable. It was hewn, and the part, which showed the natural bend, or bow form, cut off for the bow, split in two, and the half to be used was carved in the camp with the *parang* and shaped with a knife used for splitting *rotan*. It was shaved and made thinner, till the work-master felt the desired resistance in the bow. He tested this resistance by pressing one end of the bow against the ground, and trying to bend the other end *against the natural curve* of the wood. Then the string, of twisted *terap* (*saog* in the Jahai language) bark (*artocarpus*), was fastened to the notched ends but still against the natural curve of the wood.

I especially draw attention to it because we know something similar of the Andaman bow. The Andamanese, too, use woods with a natural bend, which then are strung in the opposite direction.

The transverse section of the Semang bow is . I suppose, therefore, that originally the bow was of split bamboo, but strengthened with wood. Then the raising on the inner side of the bow staff gives a suggestion of the inlaid strengthening wood. Otherwise we do not see why it is specially cut out. Semang bows of bamboo are in the Berlin Museum, but I have not seen any myself in Malaya.

The quiver is made of *buloh minyak* (Jahai *genum semei*). About the arrow Skeat says the following (pp. 274-75): “ But the extraordinary part about the “ feathering of these arrows is that the web of each feather is clipped right up to “ the quill, so that it can have only the very slightest effect upon the flight of the “ arrow. Moreover the two webs are affixed (all that is left of them) at a convergent “ angle, and the question which at once suggests itself, in view of this peculiar “ method of fastening them, is whether the Semang really understand the principle of “ feathering, and whether they do not rather employ it either as the mutilated survival “ of more intelligent methods, or perhaps make use of it for solely magical “ reasons.”

Full of interest is the explanation of this fact given to me by some Semangs. When the arrow is shot at some game—in my case they mentioned a wild boar—it whistles in its flight owing to the clipped feather, and it whistles the more if the feather is applied in the opposite direction because the resistance to the air is greater. The wild boar hears the whistling, stops frightened and looks about, and it is hit more easily.

That is the reason of the application of such a feather, and I believe that the same idea is at the root of the reason for applying a leaf or a piece of leather to the shaft, as we are told the Pygmies of Africa do.

Thus this feathering would have nothing to do with the feathering for the purpose of flying, as we find it with other people, and as it was supposed to be with the Semangs. On the contrary they seem to lean towards the Siberian arrows, in which a hole is made in the shaft to produce a sound in flying.

This must be the purpose of the feathering of the Semang arrow, and probably of the arrow of the Pygmies of Central Africa. R. SCHEBESTA.

America, Central: Religion.

Lothrop.

A Quiché Altar. By S. K. Lothrop.

55

Pagan survivals of religious practices ante-dating the Spanish conquest of Middle America have been described by historians and anthropologists under the name of Nagualism.* To the outward observance of Christian ritual the natives yielded readily, the more so because most of the Spanish churches stood on the sites of ancient temples, and because often the native priests secreted images of their gods in little vaults within or under the Christian altar.† Thus a devotee, piously kneeling before the Cross, might pray to the gods of his ancestors. These measures were the more readily adopted by the Indian nobility because they hoped so to conduct themselves in the eyes of the Spaniards that they might retain their estates, and they in turn were followed by the commonalty. Moreover, Nagualism was fostered by the wide-spread and still prevalent belief that the domination of the white race was only temporary and that the Indian would some day regain his heritage.

After the destruction of Utatlan, the Quiché capital, by Pedro de Alvarado, the surviving priests and nobles—according to local tradition—moved in a body to the little town known as Santo Tomás Chichicastenango (Spanish and Nahuatl) or Tzuilá (Quiché). Here, in the seventeenth century, was found the important native manuscript known as *Popol Vuh*. To this day the inhabitants have retained a dignity and race-consciousness notably lacking in the generally sullen and unresponsive population of the highlands of Guatemala. That these Indians regard themselves as “swells” is brought out by their costumes. These are of a uniform cut and colour-scheme as in all the villages of the highlands, but each family displays embroidered patterns which are inherited like a coat-of-arms.

One son from every generation is selected by the family to be a priest-doctor. The initiation, performed in secret, occupies several days. Thereafter the initiate has definite offices. He must perform elaborate ceremonies at the birth of a child, must sacrifice a chicken over a river, and doubtless must select a nagual or guardian spirit in the manner described by the ecclesiastics of the seventeenth century. Also he performs the marriage ceremony, admonishing the couple and placing a mealing stone in the hands of the girl and a machete in the hands of the

* Important studies have been published by Brasseur de Bourbourg (in the *National Intelligencer*, Washington, D.C., 26th October, 1854) and by Brinton (in the *Proceedings of the American Philosophical Society*, Vol. XXXIII, 1894). Both are based on Nuñez de la Vega's “*Constituciones diocesanas del obispado de Chiappa*,” published in Rome in 1702.

† Stephens (in “*Incidents of Travel in Central America*,” London, 1854, pp. 311-312) describes a stone, said to have been cut from a pillar of justice at Iximché, the Cakchikel capital, and placed on the altar of the church at Teopan, Guatemala. Squier (in “*A Visit to the Guajiquero Indians*,” *Harper's New Monthly Magazine*, October, 1859) speaks of ancient pottery vessels concealed behind the altar in the little town of Yarumela in Honduras.

boy, as was done in the ancient rite. In sickness he is expected to work cures.* In addition, the "brujo" must perform certain elaborate ceremonies which appeal directly to the ancient gods. About these nothing is known, for naturally they are not held in the Catholic church in the market place, although there, on the steps in the early morning, you may see incense burned to the rising sun with music of drum and flute—a procedure scarcely sanctioned by Rome. It is in secret, behind the closed doors of private houses or out on the mountains, that the native rites are celebrated. Occasional altars have been reported in the hills, which have usually been left strictly alone because the natives have been known to kill desecrators.

One of these altars, of which an illustration is given (Fig. 1), stands on a mountain ridge a few leagues south of Santo Tomás, not far from the road to Tecpan Guatemala and Iximché, the ancient Cakchikel capital. The altar consists of a rectangular



FIG. 1.—A QUICHÉ ALTAR NEAR SANTO TOMÁS CHICHICASTENANGO TZUILÁ, GUATEMALA.

mass of stones with a low pit in front, flanked by crude stone walls. At the back are several crosses, placed there to mark the fact that the worshippers call themselves Christians. The upper surface is sprinkled with palm leaves and flowers, with here and there rounded stones with a hole through them. These

are ancient club heads, and, together with some stone celts and obsidian-flake knives (not visible in the photograph) have undoubtedly been ploughed up from some ruined city. Their function on the altar is to emphasise continuity with the past. The small pit in front, which serves as a fireplace, contains ashes and traces of burnt incense.

Surrounding the altar is a space which has been carefully cleared and then partly covered with freshly cut grass and flowers. Lying on the grass in front of the firepit are several maize leaves, which have been rolled up and secured. The symbolism is probably that of the prayer-stick, widely used in the south-western part of the United States. Into the maize leaf before it was rolled up no doubt a prayer was breathed, to be carried to the gods with the smoke of burning incense.

On the nature of the rites performed before such altars no information is available. Unquestionably they are as mixed in their make-up as is the visible symbolism, which recalls the appeal of a modern Quiché hymn† alike to "Jesus Christ my God," "Captain Santiago," "Saint Christopher," etc., as well as to the "chief of the Genii who dwell in the mountain of Sija-Raxquin," the "gods of the Mountain," the "gods of the Plain," the "god Quiacbasulup," and the spirits of various deceased brujos.

S. K. LOTHROP.

* Dr. Charles F. Secord, for many years a medical missionary at Santo Tomás Chichicastenango, told the writer that there was much professional jealousy among the "brujos," directed especially against himself; but that, after he had become well established, they often called him in consultation for diseases such as goitre, which they could not cure.

† An English translation is given by Brigham in "Guatemala, the Land of the Quetzal," New York, 1887, p. 417.

REVIEWS.

Congo: Ethnography.

Torday.

Causeries Congolaises. Par E. Torday. Bruxelles: Vromant & Co., 1925. pp. 233. Price, 15 fr.

56

That science and literary charm are not incompatible is proved by this little book, which, though meant primarily to inspire European residents in the Belgian Congo with more interest in their native charges, will likewise appeal to the serious student as a masterly effort of generalised description. Mr. Torday is not only a distinguished explorer but is also a member of the fraternity of anthropologists—one of those who try to view every kind of man naturally, that is, as he exists for himself. To enter another man's world and to understand it as a scheme of meanings and purposes which, for him at least, if not for oneself, are humanly satisfying is no easy matter. A rather rare sort of sympathy is required, such as submits to the control of facts, and bases its appreciation of some particular way of life on a knowledge of the special conditions which it is designed to meet.

Thus it is from the native point of view that Mr. Torday tries to judge an institution in order to discover whether it works well or badly. The position of woman, for instance, mere chattel and drudge as she seems to be according to our notions, is shown to be at all events completely consonant with her own conception of her rights; so much so that it is precisely from the woman's side that the European reformer is likely to meet with the most determined opposition. For the rest, such rights as she has are a proud possession which the whole of her sex is prepared to vindicate by means of that irresistible weapon, the shrewish sanction. Nor does matrimonial custom in all respects favour the mere male. Not to speak of the complex effects of mother-right, a father must submit to the inconvenience of *couvade*, whether it be true or not that he is suggestible enough to feel the pains that he simulates; nor dare he quarrel with his wife lest the infant's spirit depart to a more peaceful abode; nay, whereas the prospective mother may bedeck her head in the style of a chief, her lord and master must submit to having his waistcloth slit, an operation which effectively prevents him from cutting a figure in public. On the other hand, polygamy is in Mr. Torday's opinion against the interest of the majority; because the old can alone amass the wealth needed for extensive dealings in the marriage market, so that the young and vigorous have too often to go mateless. He would by no means advocate, however, a sudden enforcement of monogamy, telling a tale of what happened once when this was done, namely, of a slump in bride-prices of catastrophic dimensions, which at least is *ben trovato*.

Of chiefs Mr. Torday discourses with genuine respect for their patriarchal virtues, and it is surely by a slip of the pen that, when pointing out that they derive but slight material advantages from their services, he suggests that vanity lies at the root of their devotion. Of medicine men, on the other hand, he has less good to say, as is ungrateful of him seeing that one of these gentry discovered by divination a stolen knife of his in five minutes. He makes a sound point, however, when he insists on the ease with which white magic in the same hands can turn to black; though perhaps he does not sufficiently allow for the fact that a commination service is not black magic, though, apart from their official and licit character, the curses which it sets in motion are not unlike those inspired by private hate. But a short notice such as this cannot do justice to a thousand observations of value, still less to the felicity, ease, and breadth of the whole treatment. Not only Mr. Torday's friends who reside in the Belgian Congo, but every student of the simpler societies of mankind, will be the better for reading this book with due attention.

R. R. MARETT.

China: Anthropology.

Arne : Black.

Palaeontologica Sinica, Series D., Vol. I. Fascicles 1 and 2. Painted Stone Age Pottery from the Province of Honan, China. By T. J. Arne.

57

The Human Skeletal Remains from the Sja Kuo T'un Cave Deposit in comparison with those from Yang Shao Tsun and with North China skeletal material. By Davidson Black.

In spite of warlike Tuchans, Christian Generals, tariff squabbles and other minor and major troubles in China, the intense, almost passionate, interest which that country displays in her past has not diminished. The Chinese Geological Survey is essentially a Chinese institution, although its directors, unprejudiced men of learning, have not scrupled to call in Western aid on technical points. There can be little hesitation in offering to Messrs. Ting and Wong our gratitude for these beautiful *brochures*, which fully justify their somewhat bold venture in associating archæology and anthropology with geology. The previous publications, noticed in MAN, XXV, 1925, 10, were actual accounts of field work and excavation. These two monographs are in the nature of a stock-taking. Dr. Arne discusses the pottery. After a clear account of various technical points, supplemented in an appendix by an analysis of some sherds by Dr. Meyersburg, the author describes at some length the typology and relationships of the pottery found by Dr. Andersson. He comes to the conclusion, and it seems to be a just one, that the nearest parallels are to be found in Susa I, although he admits the close parallelism to be found in other types of painted pottery from western Asia and south-eastern Europe, including Susa II (patterns and use of the potter's wheel) and patterns and colour with Anau I and II. He does not discuss the dating at great length, but suggests that probably it should be assigned to somewhere near 3000 B.C., on the ground of contemporaneity with the western parallel cultures. Until we possess further links in the chain, absolute dating is difficult and, for the moment, Dr. Arne's reticence is perhaps wise; but it would have been very interesting if he had allowed himself space to elaborate this problem. It is made all the more interesting by the fact that the Honan ware is so eclectic, combining, as it does, the features of several different types of painted pots from widely scattered areas.

There is one small point of criticism. Dr. Arne, in calling attention to Japanese parallels, has neglected any work which has been done in that country since Dr. Munro's book of 1908. Surely the very considerable work done by the University of Kyoto, to name only one Institution, deserves at least a passing notice in this connection?

In writing his paper Dr. Black has been at the great disadvantage that his material was in a very fragmentary condition and there was, "in addition, an utter absence of normal association between related skeletal parts." He has therefore wisely limited himself to those individual bones which were capable of accurate measurement and observation. Each bone is discussed separately, under three headings—the measurements, the morphological observations, and a general summary. Unfortunately, no skulls were available. The result is a masterpiece of orderly analysis and his work will undoubtedly prove an invaluable quarry for all who are interested in the physical anthropology of the Far East. The comparisons with Amerind material are particularly instructive. To single out a single bone, perhaps Dr. Black's treatment of the sacrum may be considered of special value, including as it does a detailed analysis of Matsumoto's famous alleged Neandertaloid sacrum. Dr. Black believes that this type of sacrum is frequently found among the inhabitants of China since Chalcolithic times. He comes to the conclusion that his two groups of ancient material do not differ essentially from the modern Chinese. He suggests further that such differences as do exist can be paralleled by the differences between various groups of North American Indians, a conclusion well in accord

with Hrdlička's most recent work on the connection between Asiatic and American groups.

The small number of individuals used to find the means in the comparative material cited by Dr. Black shows how much we are in need of careful studies on the skeleton; it seems a pity, however, that use has not been made of Pearson's classic work on the femur. In dealing, for instance, with the presence and absence of the third trochanter, 20 individuals belonging to Non-Asiatic races are quoted of whom 10 per cent. had a trochanter present and 15 per cent. a fossa hypertrochanterica. Pearson, in a series of 461 ♂ Londoners, found 48·8 per cent. and 349 ♀, 57·3 per cent. possessed a third trochanter—in other words, the Sha Kuo T'un material (41·3 per cent.) instead of possessing the trochanter "relatively frequently" possesses it less frequently than 17th century Londoners, but more frequently than the modern Chinese. Pearson also found considerable differences between sex and side, Dr. Black, relying on Von Torok's observations, considers that they may be disregarded.

The miscellaneous non-Asiatic group of 20 individuals appears frequently in Dr. Black's tables. It seems to be a very unsatisfactory form of data, and is hardly worth printing, still less of drawing any conclusions from. Apart from this, however, for which it is to be hoped more satisfactory comparative material will be substituted when Dr. Black comes to review the other finds of Chinese Chalcolithic material, the monograph is of the greatest value to all students of physical anthropology.

L. H. D. B.

Archæology.

Kendrick.

The Axe Age. By T. D. Kendrick, M.A. London: Methuen & Co. 1925. Pp. 174. Price, 6s.

58

"The Axe Age" is a refreshing and stimulating book. The greater portion is concerned with the Long Barrow period, and forms an admirable thesis on that subject, but it is principally for the chapters on the Pre-Barrow period that students of Archæology have good reason to thank the author.

Numerous volumes exist that deal exhaustively both with the Old and with the New Stone Ages, but, hitherto, the No Man's Land, that which lies between the Palæolithic proper and the Neolithic proper, has always checked the most fluent of pens, with a result that the centuries between the close of the Magdalenian culture and the beginning of the "age of polished stone" usually have been dealt with in a few brief paragraphs.

Mr. Kendrick, realising that here are to be found the weakest links in the chain of European pre-history, has sought to strengthen them. The key to the position may be said to lie in the discovery of the origin, and in tracing the evolution of the epipalæolithic axe.

It is the masterly manner in which the author deals with this question that marks his work as of outstanding merit. We are given the facts relating to the subject, but the worth of a book upon pre-history must also be estimated by the skill with which the facts have been interpreted. Judged by this standard the author has achieved notable success.

Mr. Kendrick has allowed himself to abandon much of the ballast of conservatism and many instances are supplied where the form of an implement must not be taken as the criterion of culture. He is also unsympathetic with the apostles of Orientalism, dis-associating himself, in fact, from such dogmas as may be said to hamper the progress of the knowledge of pre-history. Thus freed, he offers a fresh and vigorous treatment of a perplexing problem.

Mr. Kendrick tends to write long and somewhat involved sentences. It is also to be regretted that so few references have been supplied, but he has set out his

material with care, and, so far as his chapters upon the axe period are concerned, except in one instance, with accuracy.

On page 155 we read that in Scandinavia the small pick and the shell-mound axe, or *tranchet*, first appeared in the Kitchen Midden period, although Professor Saraauw in *Prähistorische Zeitschrift*, 1914, pp. 1-28, and Friis Johansen in *Mém. Soc. Antiq. du Nord*, 1918-19, conclusively proved that both types were in use in Maglemose times.

The views held by Mr. Kendrick may not be our own, but the treatment of his hypothesis is so forceful that it must command our attention. Time, with its further evidences, has yet to pronounce judgement.

J. P. T. BURCHELL.

Religion.

Needham.

Science, Religion and Reality. Edited by Joseph Needham. London: The Sheldon Press, 1925. Pp. 396. Price, 12s. 6d. net.

59

This volume consists of ten essays, each dealing with some aspect of the relations of science and religion. Lord Balfour contributes an introduction in which, as might be expected from his general philosophic position, he inclines to find a solution of the problem in an unresolved dualism; and Dean Inge provides a conclusion in which he summarises critically the general outlook of all the essayists in the light of his own intellectual idealism. Of the essays themselves, the three—by Dr. Malinowski on "Magic, Science and Religion;" by Dr. Singer on the "Historical Relations of Religion and Science;" and by Dr. Antonio Aliota, on "Science and Religion in the Nineteenth Century"—are, in the broader sense, anthropological. Dr. Malinowski's essay in particular is at once an extremely lucid exposition of the present position and point of view of the study of primitive religion and a valuable and acute analysis of the attitude of the primitive mind in discriminating between the practical and religious aspects in the problems of everyday life.

E. N. F.

Britain: Archæology.

Bushe-Fox.

Excavation of the Late Celtic Urn-field at Swarling, Kent. By J. P. Bushe-Fox, F.S.A. Reports of the Research Committee of the Society of Antiquaries of London. Price 2s. 6d.

60

This is an admirable report on excavations carried out by Mr. C. Leonard Woolley and Mr. Thomas May in an Early Iron Age cemetery. The report contains a full descriptive list of the pottery, with many illustrations, by Mr. Bushe-Fox, an account of the brooches by Mr. Reginald A. Smith, of some iron-smelting works by Mr. Woolley, and two chapters on the political history of Europe at the time by Mr. Donald Atkinson.

Perhaps the most interesting parts are Mr. Bushe-Fox's paragraphs on the dating of the Swarling pottery, which he places between 75 B.C. and A.D. 40, on the distribution of the Aylsford-Swarling type of pottery, of which he gives a fairly exhaustive list, and certain very interesting historical conclusions.

H. J. E. P.

Religion.

Maddox.

The Medicine Man. A Sociological Study of the Character and Evolution of Shamanism. By John Lea Maddox, Ph.D. (Yale). With a foreword by Professor A. G. Keller of Yale University. New York: The Macmillan Co., 1923. Pp. xv. 330. Price, 10s. net.

61

Dr. Maddox opens with a brief sketch of those magical conceptions in primitive belief of which the specialist functions of the Medicine Man are the outcome. He then deals with the making of the Medicine Man, his functions,

methods, and position, and briefly summarises the history of some drugs. Excepting the necessary preliminaries, the Medicine Man is considered only as a therapeutic agent; the Medicine Man as priest is relegated to a later study. The author draws his material from writers who, with a few exceptions, date from the last century. His information relating to Siberia, an area of first rate importance, especially for the pathological aspects of Shamanism, is drawn exclusively from Sumner's extracts from Sieroshevski on the Yakuts, which appeared in the *Journal* of the Institute. The mass of illuminating evidence from Africa accumulated in the last twenty-five years is ignored. As the preface is dated 1921, it is to be concluded that Dr. Malinowski's valuable material from the Trobriands was not available for the author. It is hardly surprising, therefore, that his treatment of the subject does not justify his sub-title, at any rate according to present canons of sociological enquiry. The relegation of the Medicine Man as priest to a later study is a further and insuperable drawback from this point of view. Short of this, however, the book provides a useful summary, description and classification of the main facts as recorded by the authorities consulted. E. N. F.

Africa, North: Archæology.

Frobenius: Obermaier.

Hadschra Maktuba. Urzeitliche Felsbilder Klein-Afrikas. By Leo Frobenius und Hugo Obermaier. Kurt Wolff Verlag, Munich. 1925. 4to. **62**

This splendidly illustrated work, one of the publications of the Forschungs-Institut für Kulturmorphologie, contains some sixty pages of text, over fifty coloured plates, more than a hundred monochrome plates and eleven maps. The text falls into two parts—first, an account of the finds; and, second, Obermaier's survey of the archæology of the western Mediterranean, of the prehistoric rock art of S.W. Europe and of the North African art, the subject proper of the work. Needless to say it is rather a preliminary reconnaissance, which raises problems rather than solves them. N. W. T.

CORRESPONDENCE.

British Archæology.

Cunnington: Engleheart: Stone.

To the Editor of MAN.

Stonehenge: the Supposed Blue Stone Trilithon.

SIR,—In MAN, 1926, **26**, speaking of the stone at Stonehenge (No. 150) with two cup-shaped hollows, Mr. Stone says: "If this stone had been dug up in the "course of excavation on the site of a pre-historic village it would doubtless have "been agreed, without question, that the cup-shaped hollows had been formed "as mortars for grinding grain." Mr. Stone is entitled to his theory, but it is difficult to believe that any one acquainted with the results of excavation on pre-historic sites would for a moment adopt his theory of the origin of the hollows in this stone. The kind of mealing stone in use in this part of Britain, before the introduction of the rotary quern, is well known and this stone does not bear the remotest resemblance to it. **63**

In his previous paragraph Mr. Stone says: "The opinions of various authorities on this matter are given in detail by the writer in his work on Stonehenge (pp. 14-18). The conclusion there arrived at is—that the cup-shaped hollows "were probably the work of pre-historic squatters on the site when Stonehenge "was already in a partly ruinous condition—maybe a thousand or more years "after the date of its construction." On turning to the work referred to it will be found that not one of the "various authorities" quoted suggest or hint at the possibility that the hollows in the stone might have been made for grinding corn or anything analogous to it. The credit for the theory should be given to whom it belongs—to Mr. Stone alone. M. E. CUNNINGTON.

SIR,—There is a strong and commonsense argument against Mr. E. H. Stone's pains-taking but somewhat far-fetched contentions about the diabase stone No. 150. It is certain that this stone was once a structural part of Stonehenge; from its curved shape it cannot possibly have been an upright; it must therefore have been an impost. The notion that it may have been an altar and its holes receptacles for offerings need not be seriously considered. The form of the stone excludes this, and the late Mr. E. T. Stevens wrote at a time when the now extinct attribution of Stonehenge to sacrificing Druids prevailed. That the two small holes could be used for grinding corn is equally impossible, nor would anyone conversant with the material of a prehistoric village assign such a stone to such a use. Mr. Stone presses his argument from the closeness together of the two holes quite too far. On the reasonable assumption that this stone is a relic of a small inner structure—and the curvature of the stone would indicate that this was a ring—the mortises of its lintels may have been purposely so placed in order that the projections of the lintels might be enough to bridge the alternate spaces between the uprights. This needs no diagram to make it clear.

Col. Hawley, F.S.A., in his last report on the Stonehenge excavations (*Antiquaries' Journal*, January, 1926) considers this stone and takes it for granted throughout his remarks that it is a lintel.

GEORGE ENGLEHEART.

SIR,—In reference to letters on this subject signed M. E. Cunnington and George Engleheart, I venture to think that the writers have somewhat misunderstood my position. I am not *advocating* the "mortar theory," for which I cannot claim originality—I merely put it forward as a suggestion.

For the reasons given in detail in my book on "Stonehenge," pp. 15-17, and in my article in *MAN* in March, 1926 (26), it appears to me practically impossible that Blue Stone No. 150 could have been the lintel of a trilithon.

If anyone who believes this stone to have been a lintel will make a plan to a good scale (not a hand sketch) showing, *with reasonable probability*, how such lintel might have been placed in connection with existing stones, I shall be convinced. Until such plan is produced I hold that the "lintel theory" is not tenable.

It is obviously useless for a person to advocate the lintel theory unless he can show that it is at any rate physically possible.

If the stone was not a lintel, we must seek for some other explanation for the cup-shaped hollows. Edward T. Stevens, the well-known Wiltshire archæologist, writing in 1870, notes as follows:—

"Holes were sometimes worked in detached masses of rock, or even, when in convenient positions, in rocks *in situ*, by the process of 'pecking.' These were used as mortars in which maize was crushed." ("Flint Chips," pp. 572-73.) Specimens of these mortars, not being portable, would of course not be found in our museums.

On this matter Admiral Boyle T. Somerville writes to me as follows:—

"Your contentions with regard to Stone No. 150 seem to me to be perfectly just. It appears likely that it is just a fallen upright of the blue stone circle which happens to have on it a couple of 'pit-markings'—whatever these signify! Pit-marked stones are common enough, yet nobody has ever hit on a satisfactory explanation of their meaning. I know of four, all within a mile of my house in Ireland."

In regard to the shape of the stone, I do not at all agree with Mr. Engleheart. It might very well have been an upright, but is of a most unlikely shape for a lintel.

E. HERBERT STONE.

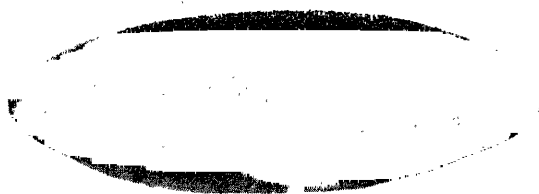


FIG. 1b.



FIG. 1.

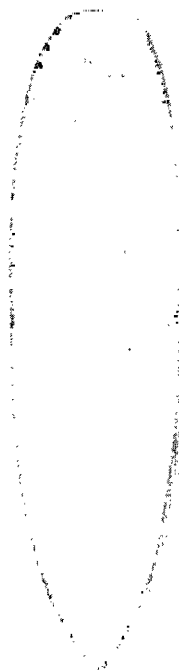


FIG. 1a.



FIG. 2.



FIG. 3.



FIG. 4.



FIG. 5.



FIG. 6.

ORIGINAL ARTICLES.

With Plates F-G.

America, South: Archæology.

Brown.

On Stone Implements from North-West Peru. By C. Barrington
Brown, M.C., M.A., F.G.S. With Plates F-G.

64

In 1911 I noticed on the surface of a small headland, Punta Picos, 26 miles south-west of Tumbes, stone flakes which appeared to have been made by human agency. Two years ago I revisited the spot with Mr. R. A. Baldry, who pronounced them to be undoubted artefacts. They are found on the sandy surface of an ancient raised sea floor, now about 60 feet above sea level. They are flakes of the simplest type and of various material, showing in every case one side as a single fracture, with typical bulb of percussion.

From that time onwards we kept watch for such flakes and began to find them at many different places: on hill tops and slopes, river terraces and plains. Nearly all had the same characteristics, but a few showed secondary pressure flaking along an edge.

About the middle of 1924 I found, at a point called El Estero, not only large quantities of these flakes, several being re-worked, but also stone implements of skilled manufacture: polished axes, pestles, mortars or bowls and other things. Again, at the head of a canyon called Quebrada Pozo Siches, among other material I found a piece of spotted slate worked into a form like a phallus; whether it is actually one is doubtful, for some archæologists to whom I have shown it do not agree.

In three places flakes were found with comparatively modern Inca remains, but it is likely that ancient sites were utilised by the Incas. In the great majority of cases no pottery fragments nor bones nor metal were found.

It is to be noted that finished implements have so far been found only in one locality, but further exploration will doubtless bring to light more such implements as well as many more occurrences of flakes.

Regarding the age of these implements, little can be said except that the makers were of a race earlier than the Incas, on account of the absence of pottery.

The more important occurrences are described in detail below.

1. EL ESTERO.

This site is the most important and interesting of all. El Estero is a small pond, dried up except after rare heavy rains, 4 miles south-west of the hamlet of La Breita on a tributary of the Mancora Valley and 22 miles inland from the coast, due east of Cabo Blanco. It is on the highest point, at an elevation of 985 feet above sea level, of a low pass through the Buitre Hills, where the trail from the coast leads to the hamlet. This trail must have been used for centuries, and it is curious that the site has not been noticed before now. The natives and others have dug in nearly every situation as accessible as this where Inca pottery and middens are found, but not here. Most of the axe heads and other implements were lying on the surface a few yards from the track.

The area of the early habitation is about 1,000 square yards, though some of it must have been removed by rains, which have cut a gully into one side. The site is at a short distance to the east of the pond. The greatest thickness of accumulated reddish grey earth with charcoal specks throughout, which defines the site, is 4 feet. There were probably only two or three houses, built of wood, for there is no trace of stonework or adobe. No trace of pottery occurs. No bones, other than a piece of charred human skull from the parietal area and a piece of tibia, were found: so the inhabitants were vegetarian. A few marine shells (*Arca*,

Conus) might represent a part of the diet, but were possibly collected merely as curiosities or ornaments.

The artefacts are described under their various classes.

A. *Axe heads.*

In all there are 31 axe heads represented, of which four are perfect and one almost so; the remainder are fragments. A great variety of material was used: andesites, rhyolite, quartzite and fine grained igneous rock, many of which are of striking colours or grain and were probably chosen on that account.

The type shows a feature unique among axes and occurring here in all except one axe. The sides* and butt are filed down to a flat or slightly convex face. In another respect, also, they are remarkable: in every case but the one noted (and in fragments not showing corners) the butts are provided with more or less protuberant ears of unusual shape. In the British Museum there are only two axe heads which are at all similar, both being in the Whymper Collection from Ecuador. One, from Otovalo,† has similar ears, but is broad in proportion to its length, a character not found in these El Estero axes; and one, from Cayambe, similar in ears and shape; but neither has the typical flattened butt and sides. In the exceptional case noted above, one corner is all that remains of the axe, the angle between the side and butt, which are rounded, being 94°. Possibly, it is not an axe head at all.

There is considerable variation in size. The largest weighs 710 grams (25 oz.), the smallest 75 grams (2.6 oz.), the majority being between 100 and 250 grams.

The ratio of length (from butt to cutting edge) to width varies from 100/100 to 100/67, the commonest proportion being 100/77. The ratio of width to thickness varies from 100/49 to 100/21. Thus there are stout convex and thin flat types.

The butt face is invariably slightly convex, and in a few cases asymmetrical with the long axis. In only three cases is there a marked constriction below the ears for binding to the haft. The four perfect axes show little signs of wear on the cutting edge. All show that they were ground smooth over the entire surface, rasp lines being visible in many cases where the final polishing was incomplete. Many of the pieces show abrasions with a sharp point on the faces, as if the blunted or broken axe had been used as a dressing block. Two pieces have been subsequently used as hammers. A very few show marks of deliberate blows resulting in fracture.

B. *Pestles.*

Pieces are common, eleven being found, but only two complete ones. The material is a coarse or fine grained igneous rock: one, however, is of quartzite. The shape is approximately cylindrical; one of the complete ones has a taper and slight bending of the axis at the smaller end. Both ends were put to use. The dimensions of this one are: length, 7.1 inches; diameter, 1.3 and 2 inches; weight, 1½ lbs. The larger complete pestle, weight 3½ lbs., is made of sandstone and has a diameter of 2½ inches at one end, expanding to 3 inches at the other. In the pieces the diameter varies from 1¾ to 2¾ inches.

Some small fingers of slate showing ground facets at the end may be pigment grinders or axe polishers.

C. *Boulds.*

Many pieces were found, representing at least 12 individuals. Materials used are most commonly sandstone, but granite and yellow limestone occur. Some

* By "side" is meant the surface at right angles to the plane of the cutting edge and to the surface of the butt. The curved surfaces which give rise by their intersection to the cutting edge are described as "faces."

† Figured in "South American Archaeology." T. A. Joyce, London, 1912, p. 68 (k).

are of considerable thickness; a granite bowl, of which the two pieces are illustrated, has a maximum thickness of 2.4 inches and must have had originally a diameter of about 12 inches and a weight of about 30 lbs. Some are of more delicate structure, of a thickness of $\frac{3}{4}$ inch at the rim. Two of these show a shallow wide groove below the rim. Some internal diameters, which can be inferred from fragments, are 8 inches, 9 inches, 12 inches, 14 inches.

There is no sign of the effects of heat, so the bowls could not have been put directly over a fire. Much time and labour must have been required for the making of a bowl. Probably a boulder was selected on account of its convenient size and shape, and the interior then ground out with a pestle and sand and water. The exterior displays skill in its graceful and regular outlines, more so than the interior, the excavation of which could be accomplished to a symmetrical shape almost mechanically.

The nearest point where granite boulders could be obtained is about 7 miles distant.

D. *Hone*.

A slab of sandstone, $10\frac{1}{2}$ inches long by 2 wide and 0.9 thick, shows use as a polishing or grinding implement, and may have been used for axe-head finishing.

E. *Hammer Stones*.

Pebbles which could conveniently be held in the hand, and one of a much smaller size, were used as hammers for flaking. The largest weighs $2\frac{3}{4}$ lbs. and has been worked into a prism and polished. Two of the axes were similarly used.

F. *Polisher*.

A small water-worn quartzite pebble with a highly polished face was found.

G. *Ornament (?)*.

A piece of shell was found which had been ground down to the size and shape of the bowl of a tea spoon, and two holes bored through. Somewhat similar artefacts are found in Chili, but their significance is not understood. They may be parts of necklaces or ear decorations.

H. *Flakes*.

Innumerable flakes of a great variety of stones are scattered everywhere. The majority are scrapers, showing a clean fracture with a bulb of percussion on one face. Some have secondary chipping on the edges: one particularly neat implement of dark quartzite is in the form of a thin disc with convex and concave bulbs of percussion. A few flakes have chisel-shaped ends; some have sharp small points; some are blunt-ended fingers. A few small cores were found.

2. QUEBRADA POZO SICHES.

A high tableland, known as the Mancora Tablazo, extends over some hundreds of square miles in the region of Cabo Blanco. The surface is flat and gravelly. Deep canyons have been carved into this tableland. On the Tablazo near the head of one of these canyons, Quebrada Pozo Siches, 11 miles south of the Cape, is an area of reddish grey charcoal-bearing earth, a sure sign of human occupation. Here a fair number of flakes were found, but little else of interest except two curious stones.

One of these is a piece of spotted slate, much weathered but still showing flat patches due to grinding. The shape strongly suggests a phallus and indicates circumcision; it must be noted, however, that there is no trace of an urethra. It is not a pestle, nor is it likely to be a pot handle, for the material does not lend itself to either of these uses. Circumcision is not known in South America, but was practised in Mexico at an early date.

The other stone is a disc, $3\frac{3}{4}$ inches in diameter and $1\frac{1}{4}$ thick, with a slight boss in the middle on both faces. The bosses and rims are polished, but the rest of the surface is roughened. It is difficult to imagine a use for this implement.

A third stone of some interest is of "dog's leg" shape, hooked at one end, and is of the well-known type of grinder for use with stone tables, such as those from Nicoya, Costa Rica.

No pottery of any kind occurs. A short distance away are two semi-circles of stones, with broken pottery; these are of later, Inca, date. I have found these semi-circles in all sorts of situations—on the Tablazo, on valley floors and hill sides. Pottery is often found nearby. The semi-circles are formed of boulders piled up to the height of 2 feet or so, or of slabs of stone set upright, in the form of a semi-circle about 15 feet in diameter, the convex side always facing south-west. The natives I have questioned do not know their origin, nor appear even to be aware of their existence; but I have no doubt that they are more or less temporary shelters erected by herdsmen in earlier times. One of a pair of contiguous semi-circles on the Tablazo near Cabo Blanco had a large slab of shelly limestone with carving upon it representing possibly a face—two concentric triangles and regularly disposed holes.

Near the encampment first mentioned were found slabs of shelly limestone with bi-conical holes, 6 and 2 inches in diameter. They are presumably of the Inca period.

3. PUNTA PICOS.

A small headland 17 miles south-west of Zorritos. This is where the first discovery of artefacts was made. No other implements were found.

4. CAÑA DULCE HILLS.

Near the hamlet of Fernandez on the Mancora River. Many flakes, some large cores and hammer stones were found.

5. QUEBRADA HONDA.

Near the junction of this valley, a deep canyon, with Quebrada Faiquillal 20 miles to the north-east of the mouth of the Pariñas River, is the site of an Inca village of considerable extent; traces of some twenty stone-built houses remain. Mr. W. Thomas, when mapping this part of the country, came across it and took me to see it. We found a large number of stone flakes, some showing secondary working, amongst the *débris* of pottery and kitchen middens, but from their occurrence it could not be determined whether they were contemporaneous or not. I am inclined to think that these stones were of a much earlier date.

6. LOBITOS.

On a hill top at Lobitos, a few miles north of the Pariñas River, I found, in 1911, a piece of black pitchstone, roughly cubic in shape, with V-shaped grooves on three faces forming rough triangles. This may be a bola stone.

7. PAITA.

Half a mile from the town, along the shore towards Punta de Paita, on a small raised beach, I found a few simple worked stones of black quartzite.

Passing in review, then, the main features of the occurrence of the implements just described, one distinguishes several perplexing points.

Flakes of simple type are found with a wide distribution, from Punta Picos to Paita on the coast, from Cabo Blanco to La Breita inland, and doubtless will eventually be found beyond these points. But nearly everywhere a simple type alone occurs; at two points only have advanced types been found. At El Estero we find these primitive implements in conjunction with rather elaborate Neolithic tools and utensils and at Quebrada Pozo Siches with a few artefacts well in

advance of the simple flakes. Are we to imagine that the makers of the flakes were anterior to those of the El Estero axes, or are they one and the same people?

Pottery-making is an ancient—a very ancient art; but the flake-makers knew nothing of it. This points to a great antiquity for the El Estero men; but when one observes the degree of denudation effected by the heavy rains which fall at thirty-year intervals in this part of the world, one hesitates to ascribe too great an age to the encampment. The flake-makers were pre-Inca—that is certain; probably disappearing long anterior to the arrival of the Incas, else pottery would be much more often associated with the flakes.

Then the food these people eat was purely vegetable. Plant life is rare now, but may have been more common in the past. For what purpose are the scrapers and borers? Not for bones and hides, for El Estero man did not kill animals, nor, presumably, had he any domesticated kinds.

C. BARRINGTON BROWN.

STONE IMPLEMENTS FROM NORTH-WEST PERU.

EXPLANATION OF PLATES.

Plate F.

1. Red chert axe head, thin flat type. El Estero.
- 1a. The same, side view, showing flat "side."
- 1b. The same, butt view, showing flat butt.
2. Green andesite axe head, small stout type. El Estero.
3. Incised pebble of black pitchstone. Lobitos.

All the above are natural size.

Plate G.

4. Granite bowl from El Estero. Half size.
5. Phallus (?) of spotted slate from Quebrada Pozo Siches. Half size.
- 6a. Scraper. One face shows a convex bulb of percussion; the other face a concave one.
- 6b, c, d. Borers.
- 6e. Borer, blunt "finger" type.
- 6f. Scraper, "chisel" type.
- 6g. Scraper, showing secondary working on edges.

6a-g are flakes from El Estero. Natural size.

NOTE.—All the implements described in the above paper are now in the Museum of Archaeology and Ethnology of Cambridge University.

Assam : Technology.

Balfour.

Ceremonial Fire-making in the Naga Hills. By Henry Balfour, *M.A., F.R.S.*

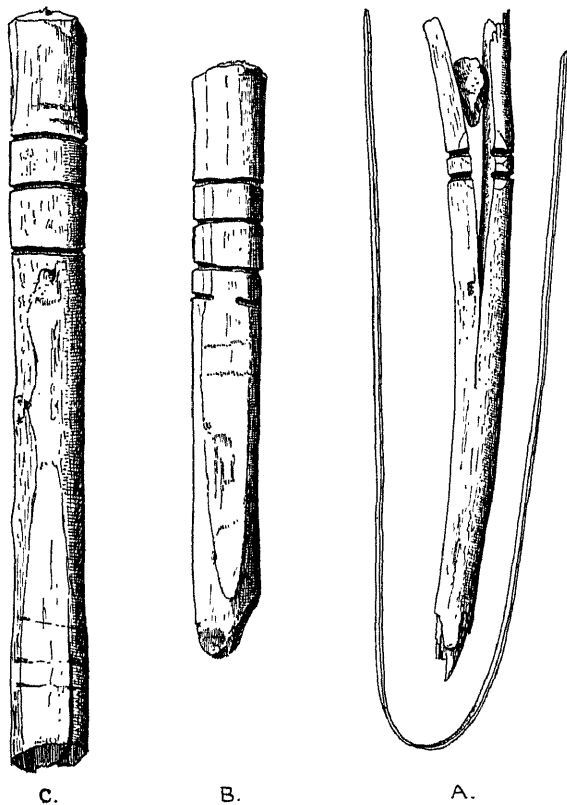
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During my ethnological tour in the Naga Hills, Eastern Assam, in 1922, I made a minor discovery which is, I think, of some little interest in connection with the practice of using frictional fire-making ritualistically, as a means of taking omens. It is, of course, well known that very many peoples, even at the present day, resort to obsolete methods of making fire, when "new" or sacred and untainted fire is required for ceremonial and religious purposes, or when divination is the objective. Amongst the Nagas the traditional and by no means yet obsolete method of obtaining fire is that of sawing a flexible cane thong round a stick of light wood, which is held down on the ground by the operator's foot; the ends of the thong being alternately pulled so as to impart a sawing motion, which, by the friction against the under surface of the "hearth" stick, generates heat rapidly enough to cause the little pile of wood-dust, churned off in the process, to start smouldering. The time required for obtaining the spark is usually from 20 to 30 seconds, as I ascertained by timing the operation on several occasions. I have described this fire-making method and its geographical dispersal fully in a paper on "Frictional Fire-making

with a Flexible Sawing-thong" (*Journ. Roy. Anthropol. Inst.*, XLIV, 1914, pp. 32 to 64). It is usual almost everywhere where this method is practised for the "hearth" stick to be split at that end which is to be subjected to the sawing-friction, a stone or piece of wood being jammed in the cleft to keep it open; and this appears to be invariably so amongst the Nagas when fire is generated for ordinary domestic purposes. I saw hundreds of used and discarded fire-sticks while touring the Hills, especially in the Sema, Ao and Lhota countries, all of which were split in the usual manner. A typical Sema example from the village of Seromi is shown, with its sawing-thong of cane, in Fig. A.* Many of the examples seen had doubtless been employed for ritualistic purposes, since these normal, cleft "hearth" sticks function for ceremonial as well as for domestic uses.

But while I was strolling about in the Ao village of Chantongia, keeping a look-out for anything interesting, I noticed lying upon the ground a discarded fire-stick

which had been used three or four times, but which was not split in the prevalent manner (Fig. B). As this was the first unsplit example which I had seen or heard of in the Naga Hills, I "collected" it with a view to making enquiries. With the kind help of my friend, Mr. J. P. Mills, who was with me, and to whom I am greatly indebted, it was ascertained from the Aos that such a stick would have been used solely for divination or taking omens; and that the reason for its not being cleft was that for purposes of augury it is unnecessary actually to procure a spark. In other words, the successful generation of fire is not essential to this *ceremonial* procedure. All that is necessary is to follow the normal procedure of the fire-making process, and to continue the sawing until the cane thong breaks. The broken ends of the thong are then examined and the exact nature of the fracture is carefully



FIRE-MAKING APPARATUS, NAGA HILLS.

studied, to see whether the omen is favourable or not. That end of the thong which was held in the *right* hand during the sawing process represents the individual or group on whose behalf the augury is taken; the end held in the *left* hand represents the antagonistic force (*e.g.*, the enemy in warfare, the game in hunting, the spirits of disease in sickness, and so forth). The relative lengths of the fibres projecting from the two fractured ends will be compared, and the omen will be reckoned favourable or the reverse, according to whether they are longer on the right hand half-thong or on the left. Other signs may also be studied in divination by this method, as, for instance, the small transverse cracks which appear upon the charred

* The figures are all one-third the actual size of the specimens.

friction surfaces of the snapped thong. These, too, help in the evaluation of the omen.

Since, then, in augury-taking by the fire-making procedure, the nature of the thong-fracture is the all-important factor, and actual ignition is unessential, it is not necessary to facilitate the procuring of a spark by splitting the "hearth"-stick after the fashion adopted where fire-production is the objective.

Such was the explanation of the uncleft fire-stick (Fig. B) picked up in Chantongia; and shortly afterwards an Ao of the same village performed the divinatory ceremony in the presence of Mr. Mills and myself, using the "hearth"-stick which is shown in Fig. C, which also is not cleft at the end.

The fact that in the Naga Hills the actual procuring of fire is unimportant in this ceremonial application of the fire-making method has a wider interest, as possibly explaining certain features noticeable in connection with the ritualistic use of this process elsewhere. Amongst the Kayans and Kenyahs of the Baram district of Sarawak, the practice obtains of resorting to frictional fire-making as a means of divination. As in the Naga Hills, the apparatus employed is the stick and flexible cane sawing-thong. Whereas the "hearth" is usually split, the thong is not invariably sawn across the cleft. One of the ritualistic purposes to which the fire-making process is applied is that associated with the ceremony of naming a child. When a name is tentatively suggested for the infant an appeal is made to Laki Pesong (who holds a dual office as God of Fire and Protector of Children), with a view to ascertaining whether the name proposed is auspicious and sanctioned by the god. For this divinatory purpose the "hearth"-stick is frequently carved in anthropomorphic form, to represent Laki Pesong; and whereas the cane thong is sometimes sawn across the cleft (which separates the legs of the figure), it is in other instances sawn round one leg only of the image, which is tantamount to employing an unsplit "hearth" (see examples figured, Figs. 7, 9, 10 and 12, in my monograph above referred to). This seems to suggest that, here again, it is immaterial whether fire is actually obtained or not; and, as a matter of fact, the augury is estimated by examination of the broken thong, as amongst the Nagas, though in Sarawak the nature of the omen is determined by comparing the lengths of the two portions of the snapped thong. It is favourable if these are of unequal length and unfavourable if their length is equal. In the event of an inauspicious equality being observed, the suggested name is deemed unacceptable and a new name is proposed, the suitability of which is tested as before. In view of these facts, it would be interesting to know whether success in obtaining a spark is unessential in divination with the fire-making apparatus among these Bornean peoples, as it is among the Nagas. This, at least, is suggested by the description of the procedure given by Hose and McDougall ("Pagan Tribes of Borneo," 1912, p. 160), though it is not actually so stated.

HENRY BALFOUR.

Caucasus: Ice Age.

Gremiatsky: Renngarten: Fleming.

The Podkouvok Skeleton. By M.A. Gremiatsky and V.P. Renngarten. Annotated from the Russian Anthropological Journal, Vol. 12, Parts 1-2, 1922, pp. 92-110 and 193-195, by Miss R. M. Fleming.

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1. ICE AGE DEPOSITS.

Piatigorsk (Lat. 44° 3' N., Long. 43° 3' E.), on the N. side of the Caucasus, stands near the river Podkouvok, which has a vast terrace about 15 metres above its left bank. The primary deposits of the region were reached by a drill through the flood plain of the Podkouvok at a depth of four metres: they were greenish-grey marls of Oligocene Age. On the first terrace the corresponding deposits were reached at a depth of 12 metres—i.e., they lay here no less than 7 metres above their

level on the flood plain, indicating the probability that there was an ancient terrace here before the glacial deposits were laid down.

The remains of a second, older terrace are very clearly shown in the prolonged Konstantinogorsk hill, composed of boulder clay and breccia. The flat top of the hill (height above sea level, 554 metres) is 64 metres above the level of the Podkoumok. At the same level there is a projection on the slope of Mt. Mashouk, where Lermontov street runs.

Still higher can be observed, here and there, the remains of a third, the oldest, terrace. On the right bank of the Podkoumok this terrace forms a plateau above the small village of Svobodni, the southern suburb of Piatigorsk (height above sea level, 596 metres), and 117 metres above the level of the bed of the Podkoumok.

In general three terraces are to be observed in the valley of the Podkoumok at heights of (1) 15 metres, (2) 60 metres, (3) 120 metres. Observations on the other rivers of the Northern Caucasus lead to the conclusion that this is a general feature.

Everywhere in the region of the foothills we encounter these three terraces, which, towards the north, are continued into pebbly layers one after another. The oldest of these layers has the greatest slope, but nowhere does it show "dislocation" in the strict sense of the word. In the region round Vladikavkaz and Nalchik we can see the unconformable superimposition of this layer upon the dislocated deposits of the Upper Pliocene (Apsheronskian layer).

Thus the age of all this series of terraces is Quaternary, Pleistocene.

The connection between these terraces and the ancient ice age complexes of the mountain chain of the Northern Caucasus was pointed out by Reinhard and Renngarten ("Beiträge zur Kenntniss der Eiszeit im Kaukasus." *Geogr. Abhandl.* Berlin, N.F., Hf. 2, 1914).

The lowest terrace is very distinctly connected in many places with the moraines of the last glacial phase. It is seldom one succeeds in finding the traces of the more ancient glaciation in connection with the deposits of the second terrace. The existence of still more ancient glaciation at the epoch when the uppermost terrace was formed has not previously been definitely proved. At any rate it can be stated that epochs of intensive deposition of broken materials in the river valleys of the Northern Caucasus have alternated three times with epochs of renovation of the river systems, and that all this has a connection with the threefold uplift of this complicated Caucasus region. With the epochs of increased fluvial deposit more or less coincided diminution of glaciation, *i.e.*, they were interglacial epochs. The layers of loess clays on the surface of the terrace are due to these clayey river-borne deposits, which were laid down at the time of the retreat of the glaciers—*i.e.*, in the epochs of a warmer and drier climate.

Let us try now to define a little more accurately the age of the deposits in which were found the human remains at Piatigorsk. The boulder-clays which form the lower part of the lowest terrace (ht. 15 metres) were deposited in the epoch of the maximum development of the glaciers of the last glacial epoch.

The traces of activity of these glaciers can be observed, incidentally, in the upper parts of the Podkoumok and its tributaries, beginning approximately at a height above sea level of 1400–1500 metres. Numerous observations and calculations of the degree of the lowering of the snow limit in the last glacial epoch compared with the present limit have been made by A. L. Reinhard for the Central and Western Caucasus. They led him to conclude that this lowering is very near to the corresponding lowering of the snow limit in the Alps in the Würm Ice Age. Thus the time of the deposit of the boulder clay on the lowest terrace of the Caucasian rivers must be considered as synchronous with the maximum development of the Würm Ice Age. This maximum stadium was accompanied or followed by a general raising of the regions, with a resultant subsequent deepening of the

river beds to their present level. The moraines of the subsequent stage of the glaciers' retreat are generally lower than the valley bed of the maximum stage. The loess clays which contained the human remains at Piatigorsk were formed from the diluvial material which was deposited on the stratum of boulder clay in the short interval of time between the beginning of the glacial retreat and the moment of the raising of the mountain region. The comparatively warm and dry climate of this intermediate period contributed to the accumulation of alluvium. In the chronology of the Quaternary epoch of Western Europe, the "Achen-schwankung" of the Alps corresponds to this period and also the Aurignacian and Solutrean of the Palæolithic Age.

Now let us try to explain the situation of the deposits in which we are interested with regard to the chronology of the quaternary deposits of South Russia. According to the observations of A. L. Reinhard on the Black Sea shores of the Caucasus and the valley of the Maïmti, the moraines of the maximum phase of the last glaciation are connected with the lowest boulder clay terrace of fluvio-glacial origin which at the sea-shore forms a marine terrace at the height of 15 metres. N. A. Grigorovitch-Beresovski (see "Les Dépôts post-pliocènes marins sur les Bords de la Mer noire" *Sapisky der Neurussischen Naturforschergesellschaft*, Bd. XXIV, Lief. 2, S. 114, 1902) thinks that in these terraces the molluscan fauna are of Mediterranean sea habitat, such molluscs being unknown in the earlier deposits of the Black Sea basin. Thus it seems that the formation of the Bosphorus and the Dardanelles Straits coincided with the end of the maximum phase of the last glaciation in the Caucasus.

N. Andrussov (in "Bosporus u. Dardanellen," *Annuaire Géol. et Mineral. de la Russie*, Vol. xii, livr. 7-8, 1910, p. 206) considers that the straits were formed following the previously mentioned lowerings in the region of the Propontis. These sinkings, apparently, occurred not only in the whole region of the Black Sea, but also along parts of the coasts (the coasts of S. Russia and the Black Sea coast of the Caucasus), causing the formation of estuaries and lowering the beds of the valleys beneath the level of the sea.

N. A. Sokolov (in "Der Mius-Liman und die Entstehung der Limäne Süd-Russlands," *Verhandl. d. k. Russischen Mineralog. Gesellschaft*, Bd. XL, S. 107 und Tabelle, 1902) places the time of the formation of the estuaries in S. Russia at the third (last) glaciation of North Russia. In the preceding epoch of increasing erosion (the second interglacial epoch) the river beds were worn down lower than the present sea level, thanks to the absolute lower level of the Black Sea (so thinks Sokolov), or thanks to the raising of certain shores, in the second, more ancient, epoch of renovation of the river system of the Caucasus. Preceding this epoch in South Russia were laid down the river sands, with pebbles and brown clay, with *Elephas primigenius*, transmitted from the middle course of the Dniepr with boulder clay (moraines of the second glaciation). Even more ancient deposits in S. Russia show layers of sand and gravel, with *Elephas antiquus (trogontherii* Pohl.), *Paludina diluviana*, *Lithoglyphus naticoides*, *Dreissensia polymorpha* and *Corbicula fluminalis*.

Hence we conclude that the time when the conglomerates and clays of the lower terrace containing the human bones of Piatigorsk were accumulated was the same as that when the estuaries of S. Russia were flooded and when the third glaciation occurred in N. Russia. We can now consider it as positively established that this glaciation corresponded to the Würm Ice Age of the Alps.

2. THE SKELETON.

The main points about the morphology of the skeleton are outlined below. The parts discovered were:—(1) part of the calotte, including almost the whole

frontal bone, the front part of both sides of the sinciput, and a small part of the nose; (2) the broken lower jaw-bone of the right side, with 5 teeth which had the crowns broken off; (3) two broken temporal bones, with the external auditory ducts; (4) the articulatory processes of the left half of the lower jaw-bone; (5) the broken left shoulder-bone, with its distal parts but without the articulation for the muscles; (6) various small broken bones.

The calotte showed the upper margins of the eye sockets, and the nasion and forehead-nasal suture were entire. Both zygomatic processes were broken off almost at the limit of the zygomatic frontal suture. The greatest lateral distance between the cheek bone processes in a straight line was 112 mm. On the right supraciliary arch are damaged bones of the region of the incisuræ supraorbitalis and a breakage on the left side revealed the dimensions of the frontal sinus. As regards sex, it seems probable that the remains are those of a female, judging from (1) the small proportions of the whole skull and the character of the individual bones; (2) the significant thinness of the bones; (3) the smoothness of contour; (4) the flattened top of the vault; (5) the angular transition between the forehead and the top of the vault. Against these features may be set the remarkable prominence of the supraciliary arches; (2) the almost complete absence of the tubera frontalia; (3) the probably great interorbital distance. These three points



FIG. 1.

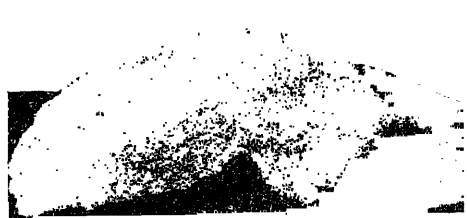


FIG. 2.

are, however, characteristic of a racial type and comparisons with Spy II and Gibraltar, which are presumably female, lead to the conclusion that Podkouvok also is female.

From the condition of the sutures and the thinness of the bones it may be concluded that the skeleton was that of a person between 50 and 70, and, most probably, between 55 and 65.

An examination of the frontal bone showed that the limit between the cerebral and orbital parts of the frontal bone was marked by the appearance of a clearly perceptible hollowing, going from the right edge of the bone to the left, more visible on the left lateral part than on the right. On the sagittal section this furrow, or depressio supratoralis (fossa supratoralis of Klaatsch), which gives to the Podkouvok skull a characteristic animal-like appearance, measured $\frac{1}{2}$ a centimetre. The following tables give a comparison of this feature in the Podkouvok skull and other skulls:—

					Angle of Depression.	Index of Depression.
Chimpanzee	-	-	-	-	139°	16·6
Neanderthal	-	-	-	-	159°	9·8
Spy II	-	-	-	-	150°	12·8
Podkouvok	-	-	-	-	155°	9·8
Many modern skulls	-	-	-	-	180°	—

Gremiatsky considers the presence of this depressio supratoralis as a specific character of the Neanderthal group.

The glabellar part of the forehead shows a distinct torus supraorbitalis, an uninterrupted ridge going from one zygomatic process to the other. The thickness

of this torus from above to below is 21 mm. at the glabella, 16 mm. in the first third of the upper limit of the orbit reckoning from the glabella, 11 mm. at the centre and 10 mm. where it passes into the zygomatic processes.

The tubera frontalia exhibit curious features in this specimen. They are extremely faintly marked and are arranged much nearer to one another than is usual. In Spy II this distance is 50 mm., in Neanderthal 40 mm., in Podkumok 52 mm., while the more usual measurement is 60 mm. If both frontal bosses are joined by a straight line, the region behind them in young subjects is usually covered with hair. This detail is mentioned as it brings out the curious appearance it would give to the face of the owner of the skull, with its narrow retreating forehead, its forward jutting eyebrow region and its wide interorbital distance. An elevation corresponding to the crista frontalis medialis present in the skulls of apes, is clearly visible in this skull, as in *Pithecanthropus* and Neanderthal.

An attempt was made to get the interorbital distance and the interorbital index. In modern races the distance is between 18-31 mm., usually 24-25 mm. In Podkumok it is probably 32 mm. The index for Spy I is 28.3 to 29.3, Neanderthal 28.8, La Chapelle 26.8, Gibraltar 26.6, Podkumok 29.1 to 30.2, and this index is near the limit for *Homo Sapiens*.

From a general comparison of the Podkumok skull with others of Neanderthal type, it appears that, as regards the morphology of the eyebrow region, it most closely resembles the Krapina and Spy II skulls.

R. M. FLEMING.

Britain: Archæology.

Human Remains found at St. Lawrence, Isle of Wight.

G. C. Dunning.

Dunning.

By

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Three human long bones were found in June, 1923, during excavations for the foundations of the new Nurses' Home at the Royal National Hospital, St. Lawrence, Isle of Wight.

Originally a low rounded hill, composed of large blocks of Upper Greensand rock, came down to about 25 feet from the northern hedge of the main Undercliff road from Ventnor. This hill was partly cleared away, and the three bones were found at a depth of about 3 feet below the surface and some 10 feet in a horizontal direction from the bottom of the slope—that is, the bones were about 35 feet north of the hedge (Fig. 1); no other bones were found although excavations were very extensive. The bones were lying close together and at the same depth in a soil composed mainly of decayed greensand.

The three bones comprise: right femur, right tibia and left femur of one individual. The heads of

both femora and the lower extremity of the tibia have been broken off, but enough remains for the length of the bones to be estimated with accuracy.

When removing the soil from the lower extremity of the left femur, I found the *right* semilunar bone firmly wedged in the intercondyloid notch. If the left semilunar bone had been found in this position, one could suppose that it had become lodged there during some disturbance of the bones, but as the bone belongs to the right side, one is led to infer that the right hand was near to the left knee in the original position of the body.

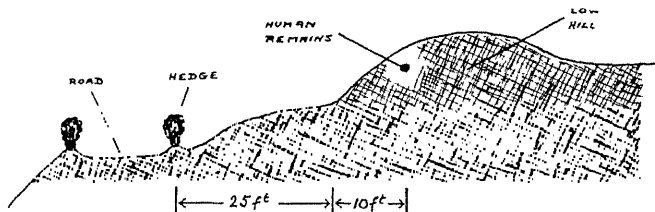


FIG. 1.—SECTION THROUGH ROAD AND LOW HILL, SHOWING APPROXIMATE POSITION OF HUMAN BONES.

The bones were submitted to Professor Sir Arthur Keith, F.R.S., who kindly reports as follows :—

“The human bones found at St. Lawrence, Isle of Wight, are the imperfect right and left femora, right tibia and right semilunar of one individual—a woman. No evidence as to the period from which they date is afforded by the manner of burial, and the condition of the bones themselves is not sufficient to indicate a date, except that they are not recent. Both femora and tibia show noticeable flattening, a condition most typical of the Bronze Age in England, less frequent in the Anglo-Saxon period, and still less frequent, though not unknown, to-day. The woman suffered from very bad rheumatism, which has affected the condyles of both femora and the head of the tibia, so that she must have walked with the knees slightly bent.”

By comparing the thigh bones with complete specimens I have estimated the original maximum length at 418 mm., and the oblique length at 415 mm. Using Professor Karl Pearson’s formulæ, the stature is calculated at about 1,540 mm. (just under 5 feet 1 inch)—a short woman.

The original length of the tibia (without including spine) is estimated at 338 mm.—81 per cent. of the length of the femur.

The following table gives the dimensions of the femora at midshaft and in the region of greatest flattening (45 mm. below the lesser trochanter), and of the tibia at the level of the nutrient foramen :—

—	Ant. post diameter.	Transverse diameter.	Index.	Circumference.
<i>R</i> —femur. Mid-shaft - - -	27	25	108·0	82
<i>L</i> —femur. Mid-shaft - - -	28	26·5	105·7	85
<i>R</i> —femur. 45 mm. below lesser trochanter - -	23	34	67·6	90
<i>L</i> —femur. 45 mm. below lesser trochanter - -	24·5	34	72·1	92
<i>R</i> —tibia. At nutrient foramen -	34·5	21	60·9	89

The mean platymeric index is 69·8. This back-to-front flattening of the upper third of the femur is well shown by comparing a section across the region of greatest flattening in the St. Lawrence femur (Fig. 2—*A*) with the corresponding section

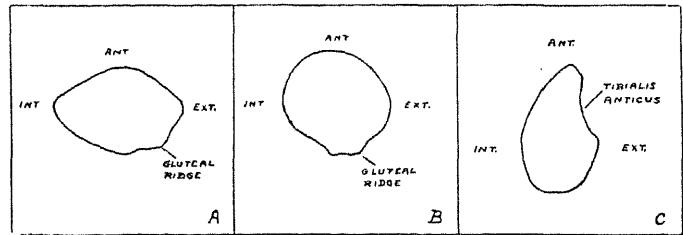


FIG. 2.—*A*—SECTION ACROSS UPPER THIRD OF ST. LAWRENCE FEMUR.
B—CORRESPONDING SECTION OF MODERN ENGLISHWOMAN’S FEMUR.
C—SECTION ACROSS UPPER THIRD OF ST. LAWRENCE TIBIA.

of the femur of a modern Englishwoman (Fig. 2—*B*). There is a well-marked flange on the outer border of the bone in the region of the attachment of the vastus externus and crureus muscles, and extending 65 mm. below the lesser trochanter. This flange is quite separate from the gluteal ridge. The flange on the inner side of the shaft is more rounded.

A section across the tibia at the level of the nutrient foramen (Fig. 2—C) shows the side-to-side flattening in the region of the attachment of the tibialis anticus muscle.

Human remains have frequently turned up quite unexpectedly in digging along the Undercliff, in most cases without any sign of deliberate burial—for example, the skeleton of a Late Celtic woman was found at Furzebrake, Steephill, in 1923, under conditions suggesting that she had been overwhelmed by a sudden landslide. Similar finds have occurred in Ventnor.

I am greatly indebted to Dr. R. C. Hutchinson, of the Royal National Hospital, for generously presenting the bones to me, and to Sir Arthur Keith for his report.

G. C. DUNNING.

REVIEWS.

Europe: Archæology.

Hoernes: Menghin.

Urgeschichte der bildenden Kunst in Europa, von den Anfängen bis um 500 vor Christi. Moritz Hoernes. Dritte Auflage, durchgesehen und ergänzt von Oswald Menghin. Pp. xix + 864. Wien: Kunstverlag Schroll, 1925. 30 gold marks. **68**

The classic work of the late Professor Hoernes, occupying 648 pages, is here supplemented by 200 pages from his successor, Professor Menghin, and the result is a massive work of reference for which students of all lands will be very grateful. Concentration of attention on the discoveries concerning Palæolithic man in France has a little delayed the unravelling of the story of man in the so-called Neolithic period, and moreover France has much less to contribute for this period until the end. It is, therefore, specially valuable that Hoernes and Menghin give each rather more than 100 pages to a largely regional treatment of Neolithic pottery. One gathers that Menghin on the whole associates himself with the growing view that in W. Europe north of Spain there was a long survival of what is rather Epipalæolithic than real Neolithic culture and that the main subsequent advance comes into that region perhaps in part from North Africa, perhaps in part from the south-east. If this review is necessarily mainly concerned with Menghin's work, this implies no lack of respect for the power of generalisation and the wide vision of Hoernes, but much of his contribution has already passed into the common stock, and he naturally takes his richly-deserved throne in the circle of the kings of prehistoric studies. Menghin, like Childe ("Dawn of European Civilisation," London, 1925), develops regional studies on a chronological basis, but, for the Neolithic, he concentrates so much upon pottery that it is at times difficult to get his views on movements of culture generally.

The divergences of culture in different parts of Palæolithic Europe is set forth in up-to-date fashion with early Capsian as the Mediterranean contemporary of the Franco-Cantabrian Aurignacian, which is held to develop into Magdalenian, while Solutrean, as usual now-a-days, is considered as a westward intrusion from the east of Europe. After the decline of Magdalenian into Azilian, the late Capsian of Spain is supposed to break through, past the Pyrenees, as Tardenoisian. Menghin is feeling his way towards the idea, to some extent shared by Childe, that east Spanish art lasted on into the Neolithic and that the Danubian Neolithic had roots in the Moravian Palæolithic—interesting new points of view which merit much consideration, but await further discoveries for their establishment. He also ventures a hint that east Spanish Capsian, Franco-Cantabrian Aurignacian, and mid-European Solutrean cultures may be associated respectively with Grimaldi, Crô-Magnon and Brunn (it would be better to say Predmost-Combe Capelle) types of man. This goes beyond the evidence, but the Grimaldi type suggests African

affinities, and the trickling of information concerning Predmost skeletons makes it highly probable that the Combe Capelle man is a western representative of that stock.

Menghin thinks that pottery was discovered once and for all, probably in some still unascertained district in Asia, and that it spread in a simple form far and wide, reaching, for example, both Predynastic Egypt and the kitchen middens of Denmark. He has many reserves about Campignian pottery. From a fairly general early type of pottery, he believes developments took place, with many mutual influences, in Crete, in Africa Minor and Western Europe (including the early Swiss lake-dwellings), in Russia and Finland (with extensions to east Sweden and west Siberia), in the northern Caucasus and the south Russian steppe, in the Baltic area, in the Danubian basin, in Asia Minor and Thessaly, and in the Ægean.

We gain an idea of peasants in the middle Danube basin advancing in agriculture and the potters' art and decorating their pots with spirals and meanders, and Menghin does not think these decorations came in from the south. He does not, however, give any hint of the facts concerning Transylvania with which Childe whets our appetite for information on origins of culture in that noted source of ancient gold. Into the Danube area Menghin brings influences from Asia Minor, *viâ* Thessaly perhaps; Childe, on the other hand, has made a strong case for interpreting the resemblances between Danubian and Thessalian cultures as the results of spreads in both directions from Transylvania. The spreads of the Danubian potters, especially westwards towards Belgium, are interesting to follow and may well account for the rise of agriculture in the west.

Menghin is inclined to agree with Åberg's version of Kossinna's views as to important autonomous developments in the Baltic, presumably the south-west Baltic area, and for a spread of influences thence towards the south-east, disturbing the Danubian cultivators on the way. Åberg's sequences may, however, be reversed, as may also those of Menghin, so it seems well worth while to think of origins of the culture in question in south Russia and of its spread north-westwards, especially over the loess areas. This would bring the movement into line with others and with the suggestions of C. E. P. Brooks, who gives the latter part of the third Millennium B.C. as a time when men would naturally move away from the drier lands. In fact, in spite of many difficulties, we see we are approaching a connected vision of a new chapter of prehistory that Peake helped to introduce in his article in the *Journal of the Royal Anthropological Institute* for 1916.

Menghin's addenda to Hoernes on the Bronze and Iron Ages are very short; he gives accounts of much recent work on the eastern Mediterranean, but his references to western Europe are slight and he hardly seems to see the interest of the sword and of the spread of its power.

The wealth of illustration and the thoughtfully-planned tables are a great help to the reader and one can only ask for the inclusion of maps in the next edition and for a little more consideration of environmental conditions and of evidences of climatic variation in connection with movements of peoples and spreads of culture.

H. J. F.

Greece : Mythology.

Bacon.

The Voyage of the Argonauts. By J. Ruth Bacon. 187 pp + 5 pls. and 3 maps. London: Methuen, 1925. 6s.

69

The Argonaut saga is brimfull of fascinating material alike for the folklorist and the prehistorian. Both have reason to be grateful to Miss Bacon for her simple yet scholarly restatement of the tale. The stages in the growth of the legend from Homer to the latest mythographers are clearly set forth and the various secondary

episodes are traced to their sources in a masterly manner. When later accretions have been thus eliminated on the principles laid down by Chadwick, the authoress finds left, not a solar myth but a kernel of historical fact—a real voyage undertaken by Ægean mariners of the Bronze Age.

For the demonstration of this perfectly just conclusion Miss Bacon should have sought the collaboration of a competent prehistoric archæologist; for when her book was written the archæological material had never been collected and could only be successfully handled by experts. Through neglect of this precaution Miss Bacon has missed most of the real evidence she wants and lost herself on imaginary amber trade routes. Since this book was written, de Navarro's paper on the amber trade and other works have appeared; hence it is allowable to hope that this vital chapter will be rewritten in the second edition, which the quality of the rest of the work merits.

V. G. C.

CORRESPONDENCE.

Ethnology.

Murray.

To the Editor of MAN.

Children of the Sun.

70

SIR,—After reading, with very great interest, Perry's "Children of the Sun," I find myself puzzled about certain matters, and venture to seek enlightenment in your pages. I have no claim to call myself an anthropologist, and I do not venture to express any opinion on the theory put forward by Mr. Perry; but I have lived for a long time in Papua and I cannot help feeling that certain statements of his are hard to reconcile with some of the facts as we in Papua have generally accepted them.

Probably some of the matters which I mention may be due to a mere oversight, and may have no bearing at all upon the theory which is sought to be established; and in others the reconciliation may be easy enough, and it may be my own fault for not having seen it at once. But I confess that I should feel more easy in my mind if the difficulties could be cleared up.

I enclose a paper in which I have enumerated the matters that are troubling me.

Yours sincerely,

Government House,
Port Moresby.

J. H. P. MURRAY.

1. *Map, p. 28.*—(a) The map does not show pearls at the Trobriands. But the Trobriands is, in fact, the only place where they have been found in any numbers.

(b) The map shows pearls at the D'Entrecasteaux Group, Bartle Bay and Port Moresby. A few have been found in these places, but their number is negligible compared with the Trobriands. Why then does the map show them? Is it because there are "archaic remains" at these places and not at the Trobriands? If so, this is surely forcing the map to fit the theory.

(c) Polished stone implements are found all over Papua, except so far as they have been superseded by steel. They are still manufactured occasionally; the method of manufacture is known and has been described. In the map, instead of being shown all over Papua, they are carefully located alongside the gold and pearls. Why is this?

(d) The map shows gold in Mekeo, on the Kikori, on the Fly, and on the Strickland. Gold has never been worked in any of these places. Why then are they shown on the map? Is it because "irrigation" or "archaic remains" are found

near these places? Colours of gold may be found in almost any river in Papua, but I think that the Strickland is an exception. Gold is shown in the map on Rossel also; it has been found there in small quantities (as in most parts of Papua), but has never been worked.

(e) Irrigation works are shown in the map at Bartle Bay, Wagga Wagga, Lakekamu, Kikori and Fly River. Of course there are none except at Bartle Bay. Perry says this himself at p. 29; but he says that there are canals in other places. The alleged "irrigation" works on the Fly are merely drains and surely can not be connected with any particular form of civilisation.

It appears to me that an enemy might say that the map has been drawn so as to fit in with the theory. Of course I know that this is not so, but the *advocatus diaboli* might urge (i) that pearls are not shown at the Trobriands because there are no "archaic remains" there—for pearls without "archaic remains" do not suit the theory; (ii) that as there are archaic remains at Port Moresby there must be pearls there also—so the map shows pearls at Port Moresby where there are hardly any, although it does not show them at the Trobriands where they are plentiful; (iii) that the "polished stone implements," which, in fact, are still in use without any particular reference to gold or pearls or anything else, are located so as to fit in with the gold, as required by the theory; (iv) that both irrigation works and gold are extended to the Kikori and the Fly, without sufficient warrant in fact; (v) that gold is shown in Mekeo, though it has never been worked there, the reason being that "archaic remains" have been found there, and in Kuni at the back, and the theory demands that gold and "archaic remains" should go together.

Possibly carelessness, or undue haste, in the compilation of the map, may be sufficient to explain all this; but I cannot help thinking that it does look, at first sight, as though the theory had, for a time, been allowed to dominate the facts.

2. The existing natives of New Guinea "neither make nor use" polished stone implements (p. 98). This is absolutely incorrect. They are still occasionally and were commonly made, and are used wherever the natives have not got steel. Perry evidently thinks that the manufacture is a lost art.

3. The people of the archaic civilisation visited Australia "while their mines of British New Guinea were being worked" (p. 99). There are no traces of old workings in British New Guinea.

4. Charms of sorcerers (p. 387). The theory is that these charms are the relics of archaic civilisation, quartz, particular kinds of stones, etc. But, in fact, sorcerers use anything that takes their fancy—stones, bones, hair, skin, sticks, herbs, cloth, tin match-boxes, bits of glass, rats' tails, etc. The head of a decanter stopper is a valued charm.

5. The natives of British New Guinea make use of the "stone images which 'the ancients left behind them'" (p. 395). They have, so far as we know, only used one stone image, that found by Meek at the Giriwu. Perry speaks as if there were dozens of them. Another one was found, but it was not in use, as it was buried many feet beneath the surface.

6. In New Guinea the "whole of the warfare apparently consists of struggles between the communities on either side of the dual organisation" (p. 491). This is quite incorrect. I have never come across a single instance that I can remember—the fighting has been usually with people altogether outside the particular organisation.



FIG. 1.—PREPARING THE CAKE OF DOUGH.

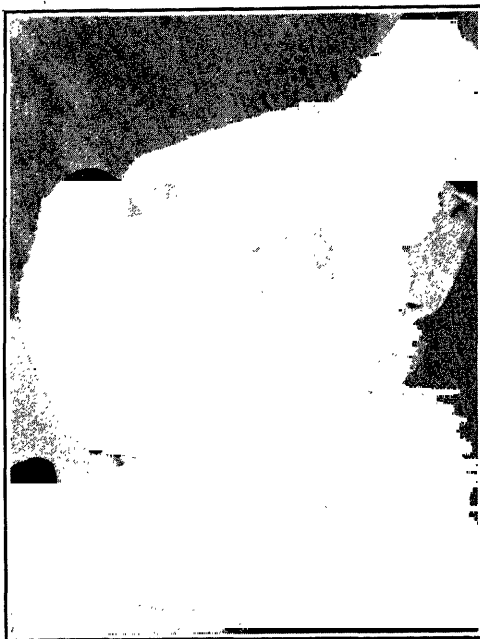


FIG. 2.—LIGHTING THE "CANDLE."



FIG. 3.—THE JAR IN POSITION, WITH THE BURNING "CANDLE" UNDER IT.



FIG. 4.—REMOVING THE JAR

A FERTILITY RITE IN MODERN EGYPT.

ORIGINAL ARTICLES.

With Plate H.

Egypt: Magic.

Blackman.

A Fertility Rite in Modern Egypt.* By Winifred S. Blackman.

71

In June, 1922, I published in *Discovery*, pp. 154-158, a paper dealing with ceremonies performed among the *felāhī* of modern Egypt to ensure offspring to childless women, or to those who, after bearing one or two children, had remained for some time without further hope of adding to their families. Last season I was fortunate enough to witness the following operation to which a woman who lives at El-Lahun in the Fayum had recourse. The woman in question is a Copt. She has been married for some time but has borne no children.

I should here mention that the performance would ordinarily have taken place in the house, but, as the light in doors was not strong enough to enable me to take satisfactory photographs, the woman consented to its taking place out of doors.

A secluded spot behind the house was selected, and a blanket spread on the ground for the woman to lie on. Her clothes were then drawn up to above her waist and the lower part of her body covered by a rug, her abdomen being thus left exposed. Some flour was put into a small bowl and mixed with water into a firm paste. One of the two women who officiated took the paste and formed it into a flat round cake (Fig. 1), which she then placed on the recumbent woman's stomach, just over the navel. Meanwhile the second woman selected a head of maize (*dura*), from which the grain had been removed, and placed it upright in the centre of the cake, where it was held in position by the first woman. Her companion now struck a match and lighted the head of maize (Fig. 2), which they called a "candle." They held this lighted "candle" in position while they placed over it a pottery jar, pressing it well into the cake. In this way the jar remained upright without further support (Fig. 3), and it was left in this position for about one minute. The Coptic woman called out more than once for the jar to be removed, but her two friends urged her to be patient and to keep the jar on her stomach a little longer.

One of the women then got up from the ground where she had been sitting and proceeded to lift the jar from off the woman's stomach. It took a very strong and somewhat lengthy pull to do this (Fig. 4), and when it was finally removed there was a resounding pop like a small explosion. It was explained to me that one possible reason for the woman not having conceived was that she had "wind" in her stomach." The noise caused by the removal of the jar was this wind, coming away, as they supposed, from the stomach through the navel. The performance was repeated two or three times, and on each occasion when the jar was pulled away the noise was very loud, and the two women remarked on how much wind she had in her stomach. As I have said, the woman who removed the jar had to exercise her full strength to get it away, and this part of the performance must have caused the patient a good deal of discomfort, if not actual pain, for her stomach was dragged up with the jar in the effort to remove it, the suction, of course, being caused by the vacuum created by placing the jar over the lighted "candle."

I was told that this is quite a usual performance for childless women in the village of El-Lahun. It is probably common to other parts of Egypt as well, but, being a somewhat private operation, it would not be talked about freely, and one has to be on very familiar terms with the people for a long time before one is invited to be present at such a ceremony.

WINIFRED S. BLACKMAN.

* The information contained in this note was obtained by me during last season's expedition, the expenses of which were mainly defrayed by the Trustees of the Percy Sladen Memorial Fund and the Royal Society.

Britain : Archæology.

Burchell.

A Settlement of the Dolmen Period. *By J. P. T. Burchell.*

72

The "occupation floor" here described is situated in Knole Park, Sevenoaks, Kent, some 300 yards east of the Golf Club House, at 485 feet O.D. To the west of it, and 85 feet below, formerly flowed the Knole River. A brief reference to the site has been made by Mr. Lewis Abbott in our Journal (1895, p. 131), and the digging of trenches across this area during the War prompted me to keep a watch on the sections thereby exposed. Frequent search from that time onwards, coupled with the information gained by previous observers, enables me to supply the following facts. The geological sequence runs :—

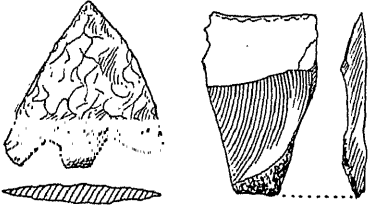


FIG. 1.
BARBED AND TRANSVERSE ARROW-HEADS,
KNOLE PARK, SEVENOAKS, KENT.

Modern turf,
Silver sand,
A layer of carstone,
Folkestone sands.

The "floor" is to be found in the silver sand and consists of :—

Flint implements,
Pot-boilers,
Calcined flints,
Charcoal fragments.

The implements may be classified under the following types :—

Polished axes,	
Barbed arrowheads,	
Transverse arrowheads,	
Scrapers,	
Borers,	} some of which are microlithic.
Blades,	
Core-scrapers,	
Cores.	

The finding of polished axes with barbed arrowheads being proved makes an association characteristic of the Dolmen period. This Whitsuntide I was fortunate in finding, *in situ*, a transverse arrowhead of a type abundant in the Shell-mounds of Scandinavia, which belong to a period immediately anterior to that of the Polished Axe. Now, in England and France, the "occupation sites" referred to as belonging to the Shell-mound Period (*i.e.*, Thatcham, Lower Halstow and Campigny) have so far failed to yield any example of the transverse arrowhead.

But in Northern France, in the succeeding, or Dolmen, period the association of polished axes, barbed arrowheads, with transverse arrowheads is firmly established. The Knole Park settlement provides a similar culture phase in Southern England. This paradox is best explained by attributing a southerly line of migration for the fully developed transverse arrowhead from the Scandinavian area.

My thanks are due to Mr. Baird, of Plaxtole, Kent, for allowing me to reproduce the barbed arrowhead in the figure illustrating this note. The scale in each instance is full size.

J. P. T. BURCHELL.

Britain : Archæology.

Crawford.

Britain during the Last Forest-Phase. *By O. G. S. Crawford, B.A.*

73

Some conclusions of geologists and meteorologists have a direct bearing upon archæology, and the implications of them do not all seem to have been fully realised.

According to Mr. C. E. P. Brooks (*Quart. Journ. Met. Soc.*, July, 1921) during the period 3000–1800 B.C. (roughly) the climate of Britain was drier than at present, while the periods immediately before and after were wetter. This was due to the northward extension over Britain of the southern anticyclone—a phenomenon which occurs occasionally at the present time (for instance, during the early part of last summer and during the whole summer of 1921). Under such conditions a barrier is set up which wards off the Atlantic depressions and thus prevents rain from falling; consequently peat does not form to any great extent.

According to geologists and botanists, during this same dry period the land stood at least 60 feet higher than at present—at any rate in England south of the Tyne, and Wales. England was connected with the Continent, and the Straits of Dover did not exist (Clement Reid, “Submerged Forests,” map on p. 40). The full extent of the emergence is doubtful, but some emergence is certain. The low-lying plain of the North Sea was covered by now-submerged forests, and by a huge fen in which the Dogger Bank may have been an island. The Humber and the Thames flowed in channels 60 feet below the level of their present estuaries; and our southern and western coasts were fringed with a forest belt where Neolithic man hunted game with bows and arrows. Brooks, in his map (*loc. cit.*, fig. 6) suggests that Wales was joined to Ireland.

I am not here concerned with the causes of these changes but with the fact of them, and with the inferences to be drawn therefrom. What, for instance, becomes of our “moist Celtic fringe”? Under prevalent anticyclonic conditions it would hardly be moist. (It is not, of course, to be imagined that such conditions were never interrupted by occasional rainy periods.) The climate of the Outer Hebrides and still more of Pembrokeshire, North Wales and Cornwall, would become very much more suited to human requirements. Crops could easily be raised and the inhabitants would be much further removed from the starvation-line than at present.

The last Forest-phase may be equated roughly with the British neolithic period, though that period may have begun before it. We can at present detect two phases of the British neolithic period: (1) an earlier phase, with no pottery, characterised by chipped flint axes with obliquely flaked (*tranchet*) edge and by numerous types of small implements, many of them resembling late palæolithic types. During this phase there is no evidence of agriculture, and the people seem to have lived by hunting on the shores of lakes and rivers; (2) a later “megalithic” phase to which belong our Long Barrows and kindred burial-places (“cromlechs” and “dolmens”) and Avebury and Stonehenge. Agriculture was known, but many of the implement-types survived with little or no change.

That the first archæological phase preceded the last peat-forming period is proved by the discovery of implements characteristic of the early neolithic industries lying upon soil under several feet of peat in Yorkshire. Peat was not forming when the floor on which they were manufactured was exposed to the air. This proves that these workshops belong to the Forest-phase; and indeed the roots and stumps of trees are also found on the same sites and at the same level as the flint implements; and in some regions of high elevation (*e.g.*, near Marsden in Yorkshire) it seems doubtful whether trees could have flourished under present climatic conditions. Further evidence comes from the western side of the island of Lewis, Outer Hebrides, where stands the Stone Circle of Callanish. The stones were set up in soil, but since they were set up peat has formed round them to a height of 5 feet. This has now been cut away, but the marks can still be seen on the stones. Thus the second or “megalithic” phase of the neolithic period—if Callanish is neolithic—fell within the Forest-phase, before peat began to form there.*

* Scottish archæologists maintain, and no doubt with good reason, that their Stone Circles are of the Bronze Age; but the evidence is derived entirely from eastern Scotland, where Stone Circles of a different and peculiar type occur.

Whence came these people of the first neolithic phase? Either they were descended from our earlier (British) palæolithic population, of which descent there is no evidence, or they must have come from the Continent of Europe. If Clement Reid's map (already referred to) be correct, they may have come on foot, or, more probably, gradually spread across the North Sea fen in canoes. Their hunting *régime* would make such an extension both easy and, indeed, almost inevitable; and it is highly significant that implements characteristic of this phase (flint axes, pigmies and harpoons) have been found in Yorkshire.* The classical sites for this phase are found in Denmark (Maglemose and Sverdborg); and the Yorkshire implements resemble the Danish more closely than they resemble any other Continental types. It is possible, therefore, that the earliest post-palæolithic inhabitants of Britain came—by infinitely slow stages, perhaps—in canoes across a vast fen from what is now Denmark.

On the other hand, it seems to be certain that the "megalithic" people (both here and on the Continent) were a distinct people altogether. They practised agriculture and their cultural connections are with the south rather than the east.

There is good evidence that the climate of Germany during the early Bronze Age, and perhaps before, was very dry; and it is generally agreed, I believe, that the most easterly European megalithic burial-chambers (the Kujavish graves of Central Germany) are of late type, and that their makers came from the north and west. But it has not hitherto been remarked that a similar phenomenon can be observed in England and Wales. The orthodox arrangement of megaliths in the British Isles in order of development is (1) simple burial chambers ("dolmens"); (2) passage graves; to which may be added (3) false passage graves; and (4) long barrows with cists. But the simple dolmen is rare, except in the extreme west (West Wales and Cornwall). Passage graves occur in Cornwall and Wales, and they or their equivalents occur in Ireland and probably also in Argyllshire. In the Cotswolds passage graves occur, but are much less numerous than false passage graves, while simple "dolmens" are not found at all. [In Wessex most of the "megalithic" burial places are, so to speak, of earth (long barrows)]. It would thus appear that the long barrow people of Gloucestershire came to these regions from the west. The eleven long barrows round the Black Mountains of Brecknockshire are almost all of a very late type (class 4).

Evidence for an eastward migration occurs also at Stonehenge. The two hypotheses put forward to account for the bringing of the foreign stones from Pembrokeshire are (1) that they were brought back in triumph by Wiltshire-men after a raid into Wales; (2) that they were carried—as Lares and Penates—by a migrating tribe. In passing, attention may be called to the Irish character of the Middle Bronze Age pottery of Pembrokeshire, already observed by Dr. Wheeler. The resemblance between the Irish and West Welsh pottery is indeed very striking, and seems too prevalent to be due merely to trade or occasional raids. It suggests that Pembrokeshire, in the Bronze Age as in historic times, was inhabited by people who came from Ireland—that it was a "little Ireland" beyond St. George's Channel, just as afterwards it became a "little England beyond Wales"; it suggests that the Irish origin of the foreign stones may after all be ethnographically correct, if this *liaison* with Ireland was of long standing.

If it be a fact that megaliths of the earliest types occur more commonly on the western fringe than inland, an eastward migration or drift would seem necessary, and would account for the preponderance of later over earlier types in eastern Wales and in England south-east of the Severn and south of the Humber. The long barrows (of earth and stone) in this part of England—nearly 250 altogether—are

* After writing this I find that Mr. Gordon Childe has arrived independently at the same conclusions as are suggested here.

just as much part of the "megalithic" culture as the so-called "dolmens" and "cromlechs" of the west; and any theory which deals with the one group must deal also with the other.

The eastward migration would have occurred at the close of the period of dry summers, when the western lands were already becoming appreciably—perhaps uncomfortably—moister and when agriculture was, therefore, becoming difficult. Was this the cause of the migration? The newcomers would have found ideal corn land in Wiltshire where, even during the very wet period—wetter even than the present—of the Early Iron Age, thousands of acres in Wessex produced annual crops of corn. The long barrow people were in any case the first permanent settlers in Wessex. When they arrived they must have found the uplands of Salisbury Plain uninhabited, for the hunters of the earlier neolithic phase kept to the valleys and less open country where the game was most abundant; and in those days the dry chalk valleys must have been drier even than they are now. During the height of the dry period the chalk downs and the oolitic uplands of the Cotswolds may even have been too waterless to support human life.

It is tempting to speculate upon the causes which sent out the megalithic peoples from their southern homes to Britain. Were those causes, too, climatic? To give even a speculative answer to this question we need to know what the climates of France and Spain were like when the anticyclone extended northwards and made even the climate of Britain a pleasant one. Perhaps the meteorologists will tell us.

O. G. S. CRAWFORD.

Technology.

Harrison.

Inventions: Obtrusive, Directional, and Independent. By H. S.

74

Harrison, D.Sc.

The discussion of Diffusion in its wider aspects has of late been obscured by the dust of conflict round the Children of the Sun, who resist attempted applications of tar and feathers. Ground for a more general controversy is still available, however, since there exists a broad boundary, with a fence, between those who readily appeal to independent invention when in need, and those whose need is never so great that they will face the abhorrent possibility. A few observations by a "diffusionist" may not be out of place in relation to this question, even though they contain nothing that is new, and are purposely restricted in their scope.

(a) Definitions are elusive abstractions, and it is difficult to arrive at any understanding of the term "invention" that would be acceptable to both the advocates and the opponents of "independent" invention. The bow, the loom, the quern, are in appearance clean-cut inventions, but it would be flying in the face of all experience, and much knowledge, to suppose that even such an apparently simple appliance as any one of these was devised by one man at a single step. The stages were no doubt few and the individual improvements slight, but the final result had a history behind it if we could only trace it. One of the steps may have been so decisive—or more than one—that we should feel compelled to allow it the title of an invention; or the changes may have each been of such a trivial nature that to none of them could we apply the term*. An aeroplane is a bundle of inventions, and so also, in its more modest but fundamental way, is the most primitive of true looms; the rotary quern, on the other hand, may include only one or two steps that seem worthy of the term. The push-quern (*e.g.*, the saddle-quern) is even simpler than the rotary type, and it is difficult to point to any step in its construction that merits the name of an invention. In any case, however, these simple appliances, and many others, had a story of development that had been completed before any history was recorded.

* See below, for observations on "variations" and "mutations."

The more complex the story, the more stages in the development, and the earlier its completion, the less likely is it that there would have been an independent repetition of the process in other areas. Conversely, the less complex the story, the less need have we to be shocked at the idea of independent origin. For example, the use of gourds and shells for water-vessels might have been discovered independently in various parts of the world; the same may be said of the use of pebbles for hammers, of splinters of stone for cutting and scraping, and even of the breaking of stone to get tools with an edge. I say "might" because there really does not seem any need to believe that they were; and this not because independent invention to that extent seems too improbable, but because the necessity for it could scarcely arise, since such simple "inventions" as these were probably made at a period when the population of the earth was confined to a small area, with a common culture. The earlier the use of a simple device, or the completion of a more complex appliance, the greater the ease of its spread from one centre over the limited area occupied by man at the time. It is unfortunately too true that the very fact of early invention usually renders it impossible to trace the steps in the development, and at the same time hampers us in our attempts to follow the course of diffusion. But because we cannot say exactly how that Palæolithic—but apparently Neanthropic—appliance, the spear-thrower, to take one example, got to Australia and America, must we conclude that it arose independently in each region? It is not necessary to go to the other extreme, and deny all possibility of independent origin, but there can be little doubt as to which is the more natural and probable assumption, and which is likely to be the more productive in research.

(b) A readiness to assume multiple origins is a striking feature of some archæological studies, and is the more surprising as work in this field is continuously providing us with the acknowledged results of diffusion. The spade of the archæologist is, indeed, digging the grave for the pallid remains of independent invention. Some investigators are prepared to credit the men of early times with a facility in invention which is directly contradicted by what we know of the reluctance of the human mind to leave the beaten path; nor do they take into account the probability that the diffusion of the fundamental material inventions outpaced the spread of the human stock or stocks which brought them forth. Independent invention must surely have been of rare occurrence in early Neanthropic times—and we may provisionally assume that it was after the emergence of this stock that the chief inventions were made—not only because of the "fortuitous" nature of the process, but because an invention once made, acquired by diffusion a right of priority in other regions, and thus forestalled the efforts of the potential foreign inventor, if such there were.

It is very easy to suggest, as is sometimes done, that the process of pottery-making (for example) is so simple, and the idea of making sun-burnt clay vessels so readily derived from observations of natural occurrences, that it may well have made its appearance over and over again. The alleged simplicity does not appeal to some of us, but, without lingering to argue the point, I should prefer to lay stress on the considerations brought forward in the last paragraph. The first pottery was probably sun-dried, and would not survive except under the most arid of conditions. At any rate, no examples have been found that suggest that they are amongst man's first ceramic triumphs. On the other hand, it has often been noted that the earliest pottery found on many ancient sites is technically superior to later examples, a fact which points to introduction followed by degeneration, or by a change of ceramic type, not always for the better. This is an argument for diffusion, and, to that extent, against independent invention, though obviously quite inconclusive. Nevertheless, let us suppose, even unjustifiably, that somewhere, once only upon a time, within easy range of the early cultures that have produced pottery far too

good to have been first attempts, there was a still earlier people who really did find out for themselves how to make pottery. Let us place them somewhere in Western Asia—but not too far west—soon after the closing phases of the Glacial Period. From such a region, and at such a time, when Europe and North Africa and America were being colonised by Neanthropic man, the conditions were at their optimum for the spread not only of the art of making pottery, but of those other arts and crafts that westerners are—or were—apt to regard as characteristically Neolithic. Why should we suppose that any of these arts and crafts should *need* to be invented more than once? There was no opportunity—the inventions got there too soon, in some cases, no doubt, carried by immigrating members of the inventive stock.

(c) The subject of pottery-making is of special interest for my purpose, since it leads to the consideration of a kind of parallelism which may well have occurred without the intervention of any “common tendency of the human mind” other than those which lead towards a better finish and embellishment. When the earliest crude pots were made and fired, the knowledge of the new art may conceivably have spread abroad before any refinements of material, shape, and decoration had been evolved. It would be carrying consistency too far, even for an advocate of diffusion, to suggest that there can have been only one occurrence of such improvements as the refining of the clay, the harder firing of the ware, the addition of impressed or incised ornament, and even (though less probably) the development of the use of slip and earth-paints. The many modifications of form to which clay so readily lends itself may also have given rise to occasional parallelism or convergence in the shapes of vessels. The point I wish to make here is, that there may be instances of independently developed similarities of composition, form, or ornamentation, which can be only disputatiously regarded as examples of independent invention. They are variations, whilst inventions are mutations.

It has never been clear to me whether the most determined of the opponents of independent invention recognise that there is any distinction such as is suggested by the foregoing analogy. Would they, for example, regard an arrowhead with a stem and barbs as so different from one with barbs and no stem, as to deserve the name of a separate invention? It is obvious that by a number of small variations in form and size a considerable change may be produced, without any new idea coming into play, and that the form finally resulting might be claimed as a new invention. Are the bronze dagger and the bronze sword two inventions or one? These and other considerations suggest that much misunderstanding arises from the commonest of causes—lack of agreement on points of terminology. Space will not permit of a full discussion of this matter, but it is clear that inventions that have arisen by the application of new ideas (mutations) are on a different footing from those pseudo-inventions which owe their origin to an accumulation of very small changes (variations). Both kinds may give evidence in favour of diffusion, but only the former could support the idea of independent invention.

(d) It is taking the bull by the horns—and perhaps by the tail as well—to venture to speak of diffusion in relation to America. It is, however, not unorthodox to believe that this continent was populated mainly from the north-west by a succession of waves of Neanthropic peoples (mixed, no doubt) bringing with them much more than the knowledge of how to hunt and trap and make weapons. Our American colleagues are reluctant to admit the possibility—still less the probability—that the proto-Amerinds were not only Neanthropic men, with Neanthropic brains, but that on earlier or later waves they brought with them all, or nearly all, of the typical appliances of an Asiatic Neolithic or Chalcolithic culture.* To oppose this

* Kroeber, for example, is a diffusionist who would put the new world in a cultural watertight compartment (see his recent book, *Anthropology*).

reluctance is not to deny the possibility of later progress, which is indeed too obvious to be denied. The Amerinds might conceivably have invented the loom, the spindle, the blow-tube, and many other things, but the opportunity did not occur, if—as is yet to be proved however—these appliances formed part of the baggage of their ancestors. In fact, if the Children of the Sun ever got to America by the cross-Pacific route, we may be sure that it was not a primitive hunting people that they encountered, eager for archaic novelties. It was a people with brains and culture that fostered an advance along lines that led them, in some areas, to a civilisation comparable with those of the Old World in ancient times, and based on the same foundations. Some things—some rotary things especially, and significantly—were missing, such as the wheel, the potter's wheel, and the rotary quern, but there was enough to enable the Amerinds to carry on the process of unfolding the possibilities of the culture which was part of their inheritance. They were capable of extending familiar inventions by variation, but the more important mutations apparently eluded them. They never, for example, converted the hoe into the plough, and the "rotary things" named above remained unknown to them.*

(e) There is a tendency on the part of advocates of independent evolution to bring forward supposed—or even actual—instances of this phenomenon in modern times. But are these relevant? There is a vast difference between the mechanism of invention as it arises out of the pooled mentality of a modern civilisation, and as it forced itself into the brain of a Neanthropic pioneer. In the one case there is a directional striving—in spite of which the investigator often discovers exactly what he isn't looking for—and in the other a mental conservatism which required an invention or discovery to be obtrusive before it could secure attention. Even in modern times we are familiar with the slowness of inventive progress, and with its crabwise advance; the occasional big jump is usually due to the combination of results arrived at along separate lines of investigation. These are days of intensive research, a recent phase in the history of civilisation; and it is obvious that in view of the world-wide spread of modern culture there may easily be similar inventions or discoveries made in distant parts of the earth—indeed it is surprising that squabbles over priority are rare enough to be conspicuous—but that they will not be examples of independent invention in the sense that this term is used in relation to the diffusion theory. They are the results of similar education, in a similar kind of artificial environment, and of directional research along similar lines, not of the response of the human brain to human needs in an approximately natural environment. (No modern invention has any relation to human needs). Primitive man, though often an agricultural labourer, was never a research student, and his inventions were the by-products of his other activities. Each discovery or invention led him on a little further, but he did not look ahead or aim at a preconceived idea of mechanical efficiency. Even in the high civilisation of Ancient China the invention of printing drifted, and was not directed, through phases which make its emergence seem to have been inevitable; but the stages were not passed through again in Europe, where block printing and, later, typography, were the results of diffusion. Since Europe happened to have an alphabet instead of a hieroglyphic system, the seed fell on soil more fertile than that in which it was first produced, and so China was easily outstripped.

If, as I contend, a parallelism in modern invention is not to be reckoned as a case of independent origin having any relevancy to the diffusion theory, the same must be said of the rare cases in which an invention of uncivilised man is independently arrived at by his betters. The fire-piston of south-east Asia, and the scientific toy acting on the same principle, and apparently invented in Europe

* The question of later Old World influence that may have affected the development of Amerind culture is obviously too thorny for incidental handling.

at the beginning of last century, may be cited. The fire-piston, we may suppose, was invented in the "natural" way, whilst the toy was the result of the experimental tastes and scientific pursuits of a section of a civilised community. It may also be said that such a device as lining a coat pocket with fish-hooks to catch a pickpocket is not an independent invention of the principle of the thorn-trap for catching fish; but the suggestion is a pleasant jest. In short, civilised man, urged on by boredom, ambition, curiosity, or what not, invents and discovers if he has the ability and the leisure, since he goes on trying till something comes of it. On the other hand, the Palæolithic, Neolithic, or Chalcolithic discoverer had his inventions thrust upon him. This has long been recognised in effect, since those who have sought to trace the origin of man's early tools and appliances have always attempted to picture some natural object or process that would suggest utilisation or imitation, or some accident or change that might lead to further progress. For the sake of a phrase, we may call this the principle of the "obtrusive invention," contrasting it with the "directional invention" of modern times. The attempt to justify this contrast is one of the objects of the present essay, since it seems to have a bearing on the diffusion controversy, and since the distinction is frequently ignored in argument. During some undetermined period (or periods) in the history of man there must have been a development of the tendency to directional research, but even now the older method has not been wholly superseded. Inventions and discoveries are still occasionally obtrusive, even in laboratories, but the primary inventions of this class were made before they could be given publication.

In conclusion, I would associate myself with those who contend that there is no firm ground for the assumption that independent invention has played any real part in human progress, and that in all cases the primary presumption is in favour of diffusion. This is a confession of faith, which others are justifying by their works.

H. S. HARRISON.

REVIEWS.

Europe: Archæology.

Childe.

The Dawn of European Civilisation. By V. Gordon Childe, B.Litt. (Oxon.). **75**
The History of Civilisation Series. Edited by C. K. Ogden, M.A. London:
Kegan Paul, Trench, Trubner & Co., Ltd. 1925. Pp. xvi + 328; 148 figures
and 4 maps. Price, 16s. net.

During the last five years a number of books have been issued dealing with the Palæolithic Age, and most of these have continued the story so as to include the Epipalæolithic or Mesolithic Age, in which most authorities now place cultures which were once considered Early Neolithic. Few of these books contain any account of later ages and their intricate problems, and such as have touched upon the subject give little but a description of some of the best known cultures and sites.

In the volume before us we have for the first time, at any rate in the English language, a serious attempt to deal with the problems of the Neolithic and Early Metal Ages. The author sets out, not to describe the doings of our savage forbears, but to tell us something of the early phases of the civilisation which was the ancestor of that which we now enjoy. The exigencies of space, and the limitations of his subject set by the scheme of which this volume forms part, have prohibited him from dealing with the first signs of civilisation in Egypt and Mesopotamia, and have prevented him from making more than passing references to Turkestan. He has, however, been permitted to describe the successive cultures found at Hissarlik, Yortan and other sites on the coast of Asia Minor.

The volume opens with a chapter on Europe in Mesolithic times, which, since the culture was but an epilogue of the Palæolithic Age, he prefers to call

Epipalæolithic. In treating it in this way he seems to think, as does a more recent writer on the same subject, that he is very revolutionary; yet most archæologists who have kept abreast of the subject have for some few years adopted that point of view. The real break, no longer to be called a hiatus, lies not between the Palæolithic and Neolithic Ages, but between the Mesolithic or Epipalæolithic and the stage which succeeded it, whether that were Neolithic, Chalcolithic or Bronze.

Though he claims to hold the balance between the advocates of *Ex oriente lux* and those of *Le mirage oriental*, the evidence he adduces is overwhelmingly in favour of the former school of thought, and he traces, stage by stage, how by two, or perhaps three routes, civilisation came from the south-east to a benighted Europe.

Owing to his familiarity with the Museum at Brasso and his acquaintance with its late Curator, whose recent death we all deplore, he has given us a very good summary of the culture found at Erösd and other neighbouring sites in the valley of the Alt. This is the first account of this culture to appear in the English language or in any general work on archæology. In opposition to some German archæologists, he argues very forcibly that this is the earliest civilisation in the Danube basin, and from it he would, perhaps, derive the Danubian culture, with its ribbon ware. He is, however, at a loss to know whence the Erösd folk derived their culture, with the knowledge of grain, painted pottery, copper and gold. This is an all-important problem for archæologists to tackle in the near future. He also traces the spread of the Danubian culture down the Rhine and into Belgium and the north-east of France.

His tracing of civilisation from Crete and the Ægean by sea to Spain and the West is not quite so clear, perhaps owing to the incompleteness of our knowledge of the Neolithic and Copper Ages in the Spanish Peninsula. The problem is also complicated by the diverse views held as to the spread of megaliths. His treatment of the further spread of culture along the Atlantic coast shows us that he is not so familiar with the scattered literature on the Bronze Age of France as he is with that of the corresponding period in the Danube basin.

On the third connection, that between South Russia and the Baltic, he speaks with a less certain voice, feeling doubtful as to the direction of the traffic which can be traced by perforated stone axes. He disposes, however, completely of the explanation offered by Kossina.

The volume is the most important contribution to the prehistory of Europe which has appeared for some years, and, though some of its conclusions are very tentative, while others will probably be modified or abandoned, it has, by gathering together all the most important items of evidence in one volume, considerably advanced our knowledge of the problems involved. No future writer on the early civilisation of Europe can afford to ignore Childe's work, on which he is heartily to be congratulated.

H. J. E. P.

Spain: Archæology.

Pericot.

La Civilización Megalítica Catalana y la Cultura Pirenaica. By Dr. Luis Pericot y Garcia. Pp. 163. Barcelona, 1925.

76

The author gives a comprehensive survey of the Catalan megalithic culture, and, in almost equal detail, an account of those adjacent cognate cultures of the Peninsula and of Southern France that the now well-known school of Catalan archæologists would like to include, together with their own homeland culture, in a homogeneous *Pyrenean* civilisation. (I have here reversed Dr. Pericot's application of the terms *culture* and *civilisation*, thinking it more in accord with

English usage to treat the first as specific and the second as generic.) To establish on a satisfactory archaeological basis this hitherto ill-defined civilisation that ranges all the southern foothills of the Pyrenees and straddles the mountains themselves at their eastern end, requires a somewhat full description of the various component elements, and Dr. Pericot deserves enthusiastic thanks for his dexterous and successful treatment of the task. His general exposition is lucid and authoritative; while, for the specialist, he summarises in a fully documented list the whole of the available information, not forgetting that important detail in the student's eyes, the present whereabouts of the objects themselves; his illustrations, moreover, are abundant and satisfactory, the most notable, perhaps, being three admirable plates whereon are grouped plans of *all* the Catalan and *all* the Basque tombs, reduced to a uniform scale. It is possible that some readers may be disappointed at the rather inadequate treatment of the skeletal material, especially the human remains from that very promising field of discovery the Catalan caves; for Dr. Pericot has only a paragraph to spare for skulls, though, of course, due reference is made to the work of Sr. Batista y Roca and others in this department. Similarly, too little is said as to the manner of the deposition of the dead, or the significance of the funeral practices observed. But in the present state of knowledge these omissions are not serious, and we should indeed be fortunate if we possessed such thoroughly competent accounts of many other megalithic cultures.

Naturally the value of the book would be lessened without a reasonable measure of debate and speculation. In this respect, Dr. Pericot is careful to be brief, but his argument is a trifle parochial and its effect is disappointing. He identifies, I think successfully, the Rousillon group of tombs with the Catalan megaliths, and even traces the same culture, in the path of the beaker, much further afield into Southern France until it disappears in a different and richer civilisation; but when he discusses whether such contact with France is in reality the result of a northward expansion of the Pyrenean civilisation, or, alternatively, whether the Catalan and Basque tombs are themselves merely the outcome of this traffic across the Pyrenees, he does not altogether dispose of the problem, though his mind is fully made up in favour of the first-named view. Admittedly, the question is a difficult one, but Mr. Leeds, arguing for the second hypothesis, approached the subject with a thoroughness (*Archæologia*, 70, 229) that deserves an equally comprehensive treatment in reply from so well-equipped an opponent as Dr. Pericot. Much turns on the origin of that tiny group of so-called *passage-graves* (*sepulcros de corredor*) isolated in the far north-eastern corner of Catalonia, and it must be confessed that even with the *revised* plans before us it still seems possible, in view of their astonishingly limited distribution, that Mr. Leeds is right in thinking them a local derivative of the *allée couverte* or *cist* form; at any rate, Dr. Pericot has no attractive rival suggestion. It may well be, however, that the author considered the scope of his book did not permit a considerable burden of detailed argument, and the lack of it certainly need not lessen our proper gratitude for this able and scholarly presentation of the facts and their attendant problems.

T. D. KENDRICK.

Britain : Archæology.

Wheeler.

Prehistoric and Roman Wales. By R. E. Mortimer Wheeler. Oxford University Press. pp. 300. Illustrated. Price 18s. net.

77

Dr. Wheeler's book cannot fail to appeal both to the student and the general reader; moreover, for those engaged in recording local antiquities it provides an admirable model.

In some 290 pages an archaeological history of Wales and her inhabitants from mid-Pleistocene times until the hey-day of the Roman occupation is clearly and succinctly given, so far as present knowledge allows.

Dr. Wheeler advances cautiously, and avoids the prevalent habit of straying afield in search of material to interpret facts.

The author is wary of accepting as truly Mousterian and Aurignacian certain implements from caves on the western seaboard, although they exhibit the facies of those cultures; and his distrust is fully justified in view of M. Peyrony's recent stratified discoveries at Laugerie-Haute, where the sequence runs as follows:—

D. Solutrian.

C. Mousterian.

B. Middle Aurignacian.

A. Upper Aurignacian.

The recording of three oval barrows, a type rare in Britain but plentiful in Scandinavia, once again raises an important question. There occur in Great Britain large numbers of implements, etc., undoubtedly Scandinavian in origin. These should be collated with the view of ascertaining what cultural influences, if any, were brought to bear by Scandinavia upon Britain between the close of the Palæolithic period and the arrival of the Beaker Folk.

It is also interesting to note the association of bronze-socketed celts with iron sickles in an Early Iron Age hoard at Llynfawr, Glamorgan.

Dr. Wheeler is shortly changing the sphere of his activities, and it is much to be hoped that he will treat of the antiquities of London in similar manner.

J. P. T. BURCHELL.

Greece: Religion.

Nilsson.

A History of Greek Religion, by Martin P. Nilsson, Professor of Classical Archaeology and Ancient History in the University of Lund. Translated by F. J. Fielden, with a preface by Sir James Frazer. Oxford: The Clarendon Press, 1925. Pp. 310. Price 12s. 6d. **78**

This is apparently the first book on Greek religion to begin with the "pre-historic" period and throughout the whole exposition to insist on the immanence of the Minoan-Mycenæan foundation in Classical religion. Yet, of course, that is the only method of obtaining a true appreciation of Hellenic religion as of any other aspect of Greek life and it is Professor Nilsson's great merit that he has so thoroughly and successfully directed religious history on to the right road. He points out, for example, that "the great cycles of myths belong to the main centres of Mycenæan culture and . . . their richness and fame are in direct correspondence with the importance of the towns in Mycenæan times." The recognition of this historical prehistoric background colours and illumines the genesis and character of Greek mythology as described in these pages.

Not that our author ignores other elements. With a wealth of happy comparisons, often taken from the folklore and mythology of his native land, he disinters the ætiological and folk-tale elements and at the same time brings out the unique originality of the Greek genius—note the absence of the wizard and the witch, elsewhere ubiquitous.

In the development of the heroic and other elements of mythology into a state-religion a great part is assigned to "Homer." "He" humanised the gods and in so doing helped to emancipate men's minds from superstitious terrors. But at the same time "he" internationalised them. Athēnē, once the divine protectress of the prince of Athēnai (with the name our author compares Mykēnai and conjectures a goddess Mykēnē) is now worshipped all over Greece and the Athēnē of

Athens cannot take the field against the Athēnē of Thebes. Hence when the several City-States came to feel the need for divine champions in their wars with their neighbours, it is the hero and not the god who come to be enthroned in men's hearts. Hence the growing importance of the hero-cults in the State religion of the fifth century. Again, through the intensification of local patriotism and *étatisme* and the rise of democracy the private cults of the nobles were broken down or, as with the Eleusinian Mysteries, taken over by the State.

Such are a few examples of our author's always illuminating and suggestive method. The only pity is that he gives so few references. And what precisely is the point of the compound "Minoan-Mycenæan"? Or why is Tiryns an older town than Mycenæ when both were inhabited in Early Helladic times and crowned with palaces by the Middle Mycenæan epoch?

V. G. C.

Africa, West: Ethnography.

Talbot.

Life in Southern Nigeria. The Magic, Beliefs and Customs of the Ibibio Tribe. By P. Amaury Talbot. London: Macmillan & Co., 1923. pp. xvi, 356.

79

This book is so rich a mine of material that it might appear ungenerous to grumble at the manner of the working. It is, however, because of the interest of the mass of evidence that Mr. Talbot has accumulated, and his undoubtedly sympathetic attitude towards the people he has so patiently studied while carrying out his work of administrator, that the anthropologist is justified in making complaints. It is almost a platitude nowadays to speak of the intercorrelation of native activities; Mr. Talbot has described numerous sacred places and beliefs, but has told us practically nothing of the social, political, or economic organisation of the Ibibio people, so that there is no solid background on which to hang his data. Thus we hear of "totemic" taboos in operation at certain places (not in certain groups), but nothing of totemic organisation, so that the relationship (if any) between the numerous beliefs in animal "affinities" and in were-animals, to totemism, as it is ordinarily understood, cannot even be hazarded. It is impossible in a short review to outline the religious ideas of the Ibibio, but it would seem that there is nothing which may not be sacred to these people, places, natural features and artificial or cult objects, animals, trees, hosts of spirits of various kinds from ancestral shades to a High God and Mother Goddess. Understanding of this complicated system is made unnecessarily difficult by the vague use of such words as "fetish" and "juju."

"Plays" form an important part in the life of the people, and many are clearly connected with the spirit world and the cult of the dead. It is doubtless difficult for anyone in the Government service to gain exact information concerning Secret Societies; however, what Mr. Talbot has to tell us is of great interest, showing the social as well as supernatural power of the members of the most important societies.

As in all studies of West Africa emphasis falls on bloodshed and human sacrifice; further, the author has been so impressed by mysterious happenings in this region that he suggests the possibility of explanation on spiritualistic lines.

BRENDA Z. SELIGMAN.

Italy: Archæology.

Holland.

The Faliscans in Prehistoric Times. By L. A. Holland. Pp. 1-162 + xiii plates. Papers of the American Academy at Rome, V, 1925.

80

This short monograph carries on the excellent work being done by members of the American Academy towards the elucidation of the problems connected with the Q. Italici. It gives a thorough account of the archæological material from

this queer little *enclave* beyond the Tiber and is adequately illustrated. The burial rites of the Early Iron Age are discussed at some length in a very conservative manner.

V. G. C.

CORRESPONDENCE.

Ethnography.

Onslow.

To the Editor of MAN.

Ethnographic Specimens in Private Hands.

SIR,—A year ago I ventured to bring before the Surrey Archæological Society a plea for the preservation of specimens of ethnographical interest in private hands.

81

The seventeenth and eighteenth centuries were centuries of discovery, but the nineteenth century was pre-eminently the century of colonisation and settlement. Comparatively speaking, the explorers and discoverers of earlier days took less interest in the civilisation and development of native races than did their successors of the nineteenth century. For obvious reasons, the earlier explorers were unable to bring home many specimens of ethnographical interest gathered during their travels. But with the advent of the nineteenth century all this was changed, and one of the main activities of that century was the colonisation of remote districts. In the work of settlement and colonisation no Power displayed greater activity than Great Britain, and it is no exaggeration to say that there is scarcely a family in the country from which some member did not go forth as soldier, sailor, administrator, explorer, trader or settler. Many of these brought home or sent home "curios" of all kinds, and with the passage of time these articles are becoming of great historical and ethnographical value. We must depend largely on collections made during the nineteenth century for evidence of the methods of life of non-European races. Nowadays the implements manufactured by the more or less primitive races have been supplanted by textiles and hardware from Europe and America, so that it becomes of increasing importance not only to preserve articles of ethnographical interest with care, but also to make a careful record of their history.

Perhaps I may give an example from my own experience of the necessity of compiling records without loss of time. Thirty-five years ago my father was Governor of New Zealand, and during his residence in that country he made an interesting collection of Maori products, such as Kiwi rugs and rugs made from phormium tenax or New Zealand flax, greenstone ornaments, *māres* or battle axes, greenstone axe heads and adzes, as well as wooden instruments and articles made from shell and bone. I have been trying to catalogue these things and to make some record of their history, but though I was in New Zealand at the time and have had the assistance of members of my family who were also there, the task has not proved an easy one. I anticipate that in twenty years' time it would have been almost impossible to have compiled any record of the history of such specimens.

I remember once seeing a wooden food bowl inlaid with shell from the South Sea Islands, but also let into the wood were two circular pieces of glass. These seemed peculiar and I asked what they were, and I was told that the bowl had been used at a feast of which the *pièce de résistance* had been roast missionary. Apparently, the reverend gentleman who figured so prominently on this occasion had suffered from short sight, and the pieces of glass had been removed from his spectacles. It is thirty years since I saw that bowl and I do not know what has become of it. Unless some record has been preserved of the origin of the pieces of glass it will be a difficult matter for the ethnologist of the future to explain.

And there is another reason for preserving records of these specimens. Races like the Maoris who were without knowledge of metals till a recent date, but who nevertheless reached a high state of civilisation, must have lived in a somewhat similar manner to our ancestors of several thousand years ago, who also were without the use of metals and, like the Maoris, used stone implements. Indeed, a comparison of stone articles from New Zealand and Neolithic specimens from European countries shows that this was the case. Such a comparison is easy if one visits the Ethnographical Collections and the Department containing the Antiquities of the Stone Age in the British Museum, so that a study of the history of ethnographical specimens of the kind I have mentioned may not be without its value in solving some of the problems of the Stone Age in Europe.

I have no claim whatever to any technical or scientific knowledge either of ethnography or of the archæology of the Stone Age, but I have had the great advantage of some correspondence on the matter with Sir Frederick Kenyon, the Director and Principal Librarian of the British Museum, and he has been kind enough to give me his expert advice on the matter.

The following are the practical suggestions which he makes to owners of specimens such as I have described:—

(1) To record on labels, firmly attached to the objects, all that is known as to their origin and the date and circumstances of their acquisition.

(2) In cases of doubt to consult the officials of the British Museum or a good local museum. The objects may be of unsuspected interest.

(3) If the owner does not wish to retain the objects, to consult the British Museum as to their most suitable destination. In some cases this will be the National Collection, in others a local museum.

It seems to me that, of these, No. 1 is the most important. Ultimate destination can be settled at any time, but the record of facts, if once lost, is lost for ever.

Clandon Park, Guildford.
29th May, 1926.

I am, Sir,
Your obedient servant,
ONSLOW.

Anthropology : Physical.

Clark.

To the Editor of MAN.

"The Chancelade Skull."

SIR,—In the study of any skull with a view to the determination of its racial affinities, a consideration of indices as well as the personal experience of the craniologist play an important preliminary part. But the final decision must rest upon a series of readily demonstrable data if it is to have any scientific value. In this question of the affinities of the Chancelade skull, we have on the one hand a fairly complete palæolithic skull, which, however, is by no means undamaged; and, on the other hand, a modern race with which the man of Chancelade is said to show convincing resemblances—the Eskimo, which possesses cranial characteristics as well defined as those of any race. In the *Journal of the Royal Anthropological Institute*, Vol. 50, 1920, I published the result of a study of a series of Eskimo skulls from old graves in Greenland. From sixteen skulls, I constructed a type contour which I hoped would be found useful for purposes of comparative craniology. I further took the opportunity of instituting a comparison between this type contour and the Chancelade skull. In order to make the comparison clear, I reproduced the type contour with Testut's figures of the Chancelade skull, both reduced to a quarter of the natural size. A study of these will at once show that there can be no doubt about the

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many remarkable resemblances between the two. For instance, one may note the scaphocephaly, the parallel lateral walls of the cranium, the "anterior dolichocephaly" with which is associated a relatively great breadth of the alisphenoids, the length and straightness of the zygomatic arch, the width of the face, the leptorhine character of the nasal skeleton, the frontal width, etc. All these and many other details can be readily seen from these contours. Other more minute features, such as the character of the nasal spine, can, of course, only be studied by reference to the individual skulls. Without, however, attempting to enter here into a detailed comparative study, it may be pointed out that there are two obvious points of difference between the Chancelade skull and the type contour. One is the shape and size of the palate. In the former the palate is long and narrow with an index of 67·9, while the average palatal index of thirty-one Eskimo skulls is 79·3. The other concerns the orbits. These in the Eskimo are large. In the Chancelade skull the orbital measurements are low. Such low measurements are found among Eskimo skulls, but, as far as I am aware, only in skulls of less than average size. The Chancelade skull, on the contrary, is relatively large. Other available skeletal characters which must be taken into account are the humero-radial and humero-femoral indices. There is a marked discrepancy between those of the Chancelade skeleton and the average of Eskimo indices.

To sum up briefly, we may say that, while the undoubted resemblances between the Chancelade remains and the Eskimo must be recognised, there are certainly three outstanding features in which they show a remarkable difference, and it would appear that these are at least sufficient to preclude a conclusive answer being given to the question—"Was the Chancelade man an Eskimo?"

W. E. LE GROS CLARK.

ANTHROPOLOGICAL NOTES.

Readership in Ethnology at the University of Cambridge.—The General Board of Studies of Cambridge University will shortly proceed to elect a Reader in Ethnology in succession to Dr. A. C. Haddon, who is about to retire. The stipend is £650 per annum, with an allowance of £200 per annum should the Reader not be a Fellow of a College. The appointment will take effect in October next and will be for a period of three years in the first instance. Applications should reach the Vice Chancellor on or before 14th July next. 83

Discovery of a Skull at Gibraltar.—It is reported in the *Daily Mail* of 14th July that Miss D. A. E. Garrod, who has been engaged in excavating at the Devil's Town, Gibraltar, has discovered fragments of a skull, including the frontal bone, which are said to present resemblances to the Gibraltar skull. The fragments were discovered at a depth of 10 feet and it is also stated that they were associated with Mousterian implements. Miss Garrod will give an account of her excavations at the Oxford meeting of the British Association in August.

Exhibition of Tardenoisian and other Microlithic Implements.—An illustrated catalogue *raisonné* of the exhibition of microlithic implements which was held at the Institute from 8th to 22nd June has been prepared and is now on sale at the offices of the Institute, price 9d., post free 10d. A characteristic specimen of each type of implement exhibited has been figured, and as the exhibits were drawn from widely distributed sites outside this country and are also representative of finds in Britain, the catalogue has a permanent value as an index of our present knowledge of the microlithic industry.

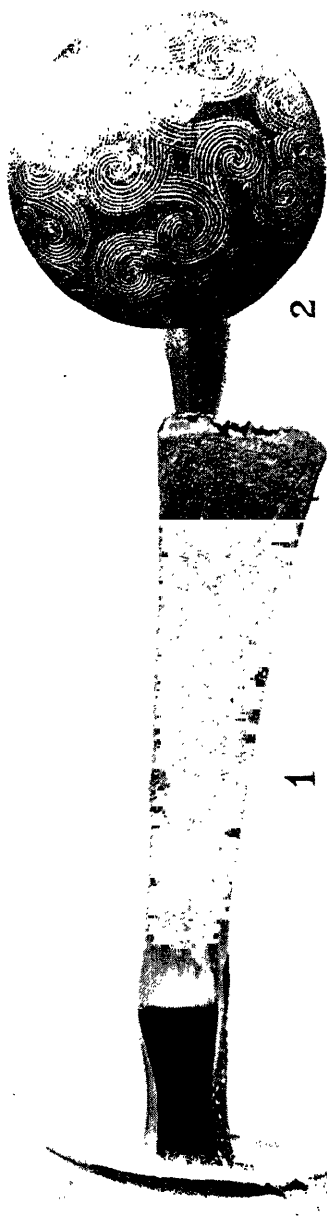


FIG. 1.—AXE FROM HAJDUVAMOSPÉCS AND BUTT OF AXE, FIG. 2, 2.

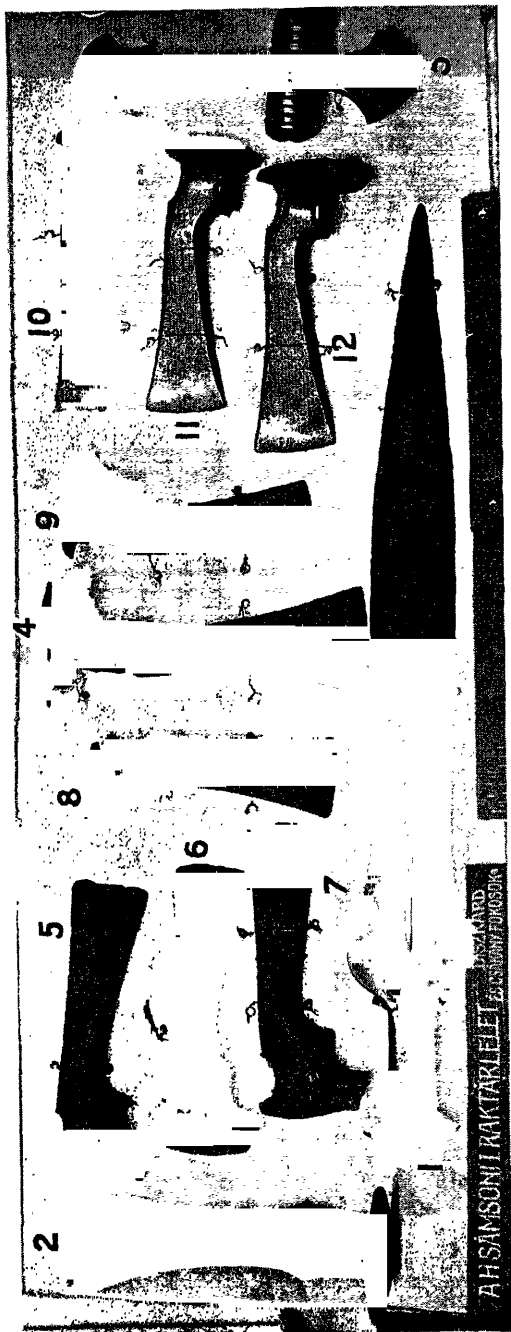


FIG. 2.—HOARD FROM HAJDUSÁMSON.
TWO BRONZE HOARDS FROM HAJDUSÁMSON.

ORIGINAL ARTICLES.

With Plates I-J.

Hungary*: Archæology.

Zoltai.

Two Bronze Hoards from Hajdúsámson, near Debreczen. By
Dr. Lajos Zoltai, Curator of the City Museum, Debreczen. (With Note by
V. Gordon Childe.)

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The finest examples of the Hungarian bronze industry come from the region known as the Nyírség, which lies within the north-east bend of the River Tisza and comprises the county of Szabolcs and part of Szatmar, Bihar and Hajdú. The Nyírség is an old pleistocene crust with ridges of drift sand running north and south, covered in the past with forests of oak and beech, and broken by depressions filled with stagnant waters once swarming with fish and wild fowl. The numerous bronze finds justify the conclusion that the Nyírség was already relatively densely populated in the Bronze Age; the marked individuality of the objects gives colour to the idea that this region was a focus of culture and industry, as Dr. András Józsa, the founder of the County Museum of Szabolcs, has cogently argued. At the same time it must be remembered that, as yet, but few cemeteries have been discovered in the Nyírség, and these were apparently but small. Our knowledge of the Bronze Age here is derived almost exclusively from stray finds and dépôts.

The Józsa Museum in Nyírgyháza, which has existed for over half a century, is peculiarly rich in such finds; but the much newer City Museum at Debreczen also possesses seven hoards. Amongst the latter, two, discovered at Hajdúsámson in the vicinity of the town, are of peculiar interest.

The first, consisting of a sword and twelve axes (Pl. I-J), was found by labourers working in the vineyards about 3½ km. north of Sámson. The objects were discovered, carefully grouped, under 75 cm. of clean sand. The sword was lying flat with the point to the south; the axes were arranged across the blade with their butts to the east. No other object was noted in the vicinity, except a small unornamented vessel (pot) with a wide rim, bulging body and small base. The regular arrangement of the weapons struck even the simple workmen. Hampel (*"Bronzkor Emlékei Magyarhonban,"* III, p. 218), commenting upon the orderly grouping of the swords at Buzita and Daróc, suggests that the weapons thus buried constituted a votive deposit. Whether our hoard was buried as an offering to gods or demons, or merely to hide it from enemies, it is a document of first-rate importance.

The leaf-shaped sword is only 53.2 cm. long; the blade measures 45 cm., and the hilt, cast in one piece with it, has a length of 8.2 cm. The butt of the blade is round and has a diameter of 7 cm. The hilt is of nearly uniform thickness and smooth; the binding coils are imitated, not by raised bands but by three groups of finely engraved parallel lines. The button of the pommel consists of closely-fitting superimposed rhombs, diminishing in area from the base, the smallest being pointed. Such a pommel is unique in Hungary, but is met on Scandinavian swords (Madsen, *"Antiquités préhistoriques du Danemark,"* II, Pl. IV, Fig. 12, Pl. VIII, Fig. 39; Sophus Müller, *"Ordning af Danmarks Oldsager, Bronzealderen,"* Pl. II, 27). Below, the hilt-plate is extended round the butt of the blade, where it is decorated with five bosses imitating rivet-heads, a feature also met on Scandinavian swords (Montelius, *"Chronologie der ältesten Bronzezeit," Archiv f. Anthropol.,* XXV and XXVI, Fig. 323). The nearest parallel to the shape of our sword is the specimen there published by Montelius, but analogous forms are figured by the same authority from North Germany and North Italy (*op. cit.*, Figs. 320 and 312).

* Throughout this article Hungary means the area comprised under that name prior to 1918 and the Hungarian names have been retained.—EDITOR.

Peculiar to Hungary, on the other hand, is the rich scroll decoration covering the whole length of the blade of the sword from Hajdusámson. Hoernes ("Urgeschichte der bildenden Kunst," 1925, p. 403) describes this pattern as the most characteristic motive of the Hungarian Bronze Age art, and regards it as a degeneration of Mycenæan ornaments.

Similar scroll patterns adorn three of the battle-axes. Two of these resemble the well-known axe from Gaura, County Szatmar (Hoernes, *op. cit.*, p. 401, Fig. 3), but the decoration is richer and more ingenious; the butt is a convex disc, but



FIG. 1.—SECOND HOARD FROM HAJDUSÁMSON—BRONZE CUPS. 1/5.

not spiked as in later specimens* (Pl. I-J, Fig. 2, 2). Unfortunately, one of these battle-axes is missing. In the third decorated axe of this group (Pl. I-J, Fig. 2, 3) the butt, though blunt, spreads out like a fan parallel to the blade, while the shaft-



FIG. 2.

SECOND HOARD FROM HAJDUSÁMSON, CAULDRON. 1/4.

hole is prolonged by a ringed tube. Battle-axes of this latter type are known from Vattina in the Banat, from Bereg, and even from Bohemia, Silesia, Bavaria and Mecklenburg-Strehlitz (Hampel, *op. cit.*, XXXI, 5, CCLV, 3; *Arch. Ert.*, 1899, p. 153; *Pravek*, 1908, p. 96; Much, "Kunsthistorischer Atlas," XXV, 1, 2, 14; von Richthofen, "Die ältere Bronzezeit in Schlesien," p. 97). These, however, are not ornamented with scroll patterns, and the blade is generally narrower than in our specimen.

The remaining axes are undecorated. One corresponds exactly to the decorated specimen with a disc-shaped butt; the other six illustrate a familiar Hungarian

type. Moulds for such have been found in the Counties of Temes and Arad, and in Transylvania.

Before leaving this first dépôt, I must emphasise one further point Hajdusámson lies near the centre of the region in which axes ornamented with the

* On the chronological development of this type of axe-hammer, see von Richthofen, *op. cit.*, p. 94.—V. G. C.

Hungarian scroll-pattern have been found. The area of their distribution is limited on the east by Gaura in County Szatmar, on the west by Puszta Szentkirály and also Némedi in County Pest, and on the south by Mezőberény in County Békés (Hampel, *op. cit.*, LXXXII, LXXXIII, LXXXIV, XXIX, and XXIV).* It is reasonable to assume that the centre of their fabrication lay somewhere in this region, embracing the central and north-eastern portion of the historical Hungary. I may illustrate here another specimen from the Debreczen Museum which was found at Hajdusámson, not far from the city (Pl. I-J, Fig. 1, 1).

The second hoard of Hajdusámson consists of hammered bronze vessels (Figs. 1 and 2). It was discovered about 1½ km. from the site of the

first find, in a vineyard at the foot of a sandhill. The vessels, three cauldrons and three cups, lay one inside the other. The peculiarity of the cauldrons lies in their double handles and the manner in which these are attached. The loops into which the handles fit project from a double T-piece fastened to the wall of the cauldron by five rivets. A ribbon bordered by small crescent-shaped marks and zig-zag lines encircles the rim of the vessel.

The cup on the right is decorated with rows of *repoussé* bosses and dots. Above the handle of the first cup a ridge with two tentacles on either side projects just below the rim.

Cauldrons and cups similar to the above formed part of the celebrated treasure of Hajduböszörmény (Hampel, *op. cit.*, LXV, 1, 3, 4). The latter hoard also included a large bronze bucket embossed with birds' heads and circles. To this vessel, too, the Debreczen Museum possesses an exact counterpart in the bucket from Szennyespusztá in the Nyírség (Fig. 3). The cups from Hajdusámson belong to the same family as the gold vases recently discovered at Budapest-Angyalföld, and remind us of types distributed from Scandinavia to Upper Italy.

LAJOS ZOLTAI.

NOTE BY V. GORDON CHILDE.

For the benefit of those readers who have not specialised in Central European prehistory, a note on the chronology of the objects described above, a knowledge of which Dr. Zoltai has taken for granted, might perhaps be desirable. The two dépôts from Hajdusámson, in fact, illustrate two periods of the Hungarian Bronze

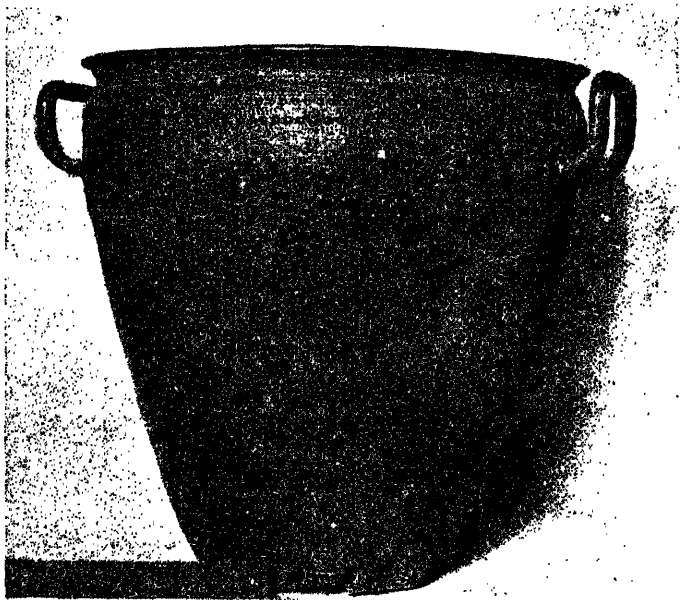


FIG. 3.—BRONZE BUCKET FROM SZENNYESPUSZTA. 1/5.

* And on the north by Rosenthal in Silesia (von Richthofen, *op. cit.*, Pl. 25, 1), and Stefkovo in Eastern Galicia (*P Z.*, x., p. 162).—V. G. C.

Age. The first belongs plainly to the Middle Bronze Age, and perhaps to its earlier phase corresponding to Reinecke's Period B. The sword provides confirmation of the inferences to this based upon the position of the axes in Reinecke's classification (*Arch. Ert.*, 1899, pp. 239-41); the Danish sword to which Dr. Zoltai compares it and which, apart from the ornamentation of the blade, it resembles in almost every detail—the three bands of fine lines and hanging triangles on the hilt, the five imitation rivet-heads, the more than semi-circular form of the false rivet-plate with its pointed ends and the midrib starting from the centre of the semi-circle of the butt—is assigned by Sophus Müller (*Mém. Soc. Ant. Nord.*, 1908, p. 13) to the first of his six phases of "l'ancien âge du bronze." The discovery of this earliest variety of the leaf-shaped sword in Hungary at an equally early date and stamped so clearly with the marks of local manufacture is peculiarly interesting: it really supplies the link missing from Peake's argument for the Hungarian origin of the type. Its occurrence in conjunction with battle-axes is also significant (*cf.* my "Dawn of European Civ.," p. 198). It is at the same time surprising that the scroll ornament regarded by Reinecke as late (*l.c.*, p. 225) should turn out to be really early; but, after all, the parallels from Period IV in Scandinavia are not very close.

The second dépôt illustrates types which are assigned by Reinecke to Hallstatt A. The dating of the group rests, in the first instance, on the precise correspondence between the motive embossed on the pail from Sennyespuszta and that on a girdle plate of Benacci I date from Bologna (Randall-MacIver, "Villanovans and Early Etruscans," Pl. 4, 6, and Reinecke, *l.c.*, p. 318). Similar pails are known from Parchim in Mecklenburg-Schwerin (*Auh.* V, V, p. 325, Pl. 56, 1018) and from Unterglauheim in Bavaria (Behren's "Bronzezeit Süddeutschlands," Fig. 9), where it was associated with cauldrons like those from Hajdúsámson. The immense majority of vessels of these two types, however, come from North Hungary (the Museum at Nyiregyháza possesses many unpublished specimens), and it seems likely that they were manufactured in that neighbourhood, although they are usually considered to be of Italic manufacture. V. GORDON CHILDE.

Africa, East: Archæology.

Seligman.

Pygmy Implements from North-East Africa. By C. G. Seligman,
M.D., F.R.S.

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Pygmy implements from Napata (Dongola Province, Sudan).

In 1919 Professor Reisner opened a number of pyramids at Kurruw on the outskirts of Napata. These proved to be the tombs of the Nubian conquerors

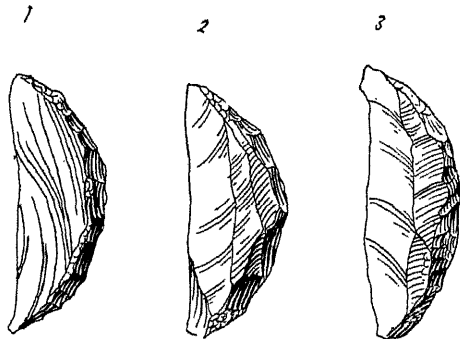


FIG. 1.—PYGMY IMPLEMENTS FROM NAPATA.
(Dongola Province, Sudan.)

of Egypt (Dynasty XXV) and their successors, *i.e.*, dating for about 100 years from 750 B.C. In these tombs were found a considerable number of lunate pygmies made of carnelian and of Tardenoisian type.

I am indebted to Mr. J. W.

Crowfoot, C.B.E., for giving me the opportunity of examining seven specimens in

this country, all less than 1.5 cm. in length, and all having worked backs. Some of these are illustrated in Fig. 1. Although only seven specimens were carefully examined, I have handled a considerable number of lunates from this site and know that at least thirty or forty were found.

Pygmy lunates from Kenya.

These consist of obsidian lunates, found in the neighbourhood of Nyoro at a depth varying from a few to 18 inches and occasionally deeper, on a site which until recently was thick forest. These are in the British Museum, and those drawn constitute Nos. 1914. 6/18. 133 to 135 and 127. Scale of figures 2/1.

I may also allude to a number of microliths of carnelian,

found at Jebel Gule, between the White and Blue Niles, and described and figured in the *J.R.A.I.* for 1910. One of these (1 b.) seems sufficiently geometrical to be regarded as a true pygmy, while on the same site a perfect core of hornstone, about an inch long, was picked up.

C. G. SELIGMAN.

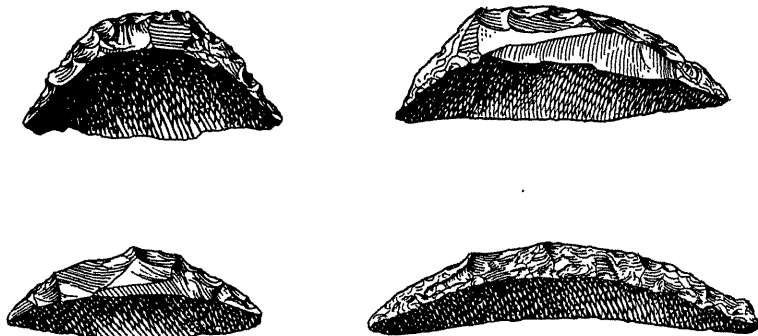


FIG. 2.—PYGMY LUNATES FROM KENYA x2.

Egypt: Religion.

Kennett.

The Sacred Litter (Mahmal) of Kharga Oasis. By B. L. Austin
Kennett.

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Kharga Oasis, situated in the Libyan Desert roughly about 115 miles west of Luxor, is an old centre of Christianity, and it may be that when the Arab invasion burst like a flood and swept over Egypt and the whole of the north coast of Africa, Kharga was at first relatively little affected. The Christian settlement at Kharga, at any rate, provided a refuge for Christians who found life too difficult under the Early Roman régime in the Nile Valley. St. Bartholomew is supposed to have preached there, and it became later a sort of penal settlement whither tiresome Christians could be quietly removed. Nestorius was banished there, and Athanasius shared the same fate, as did many others. These Christians settled almost under the shadow of the great Temple of Hibis, built by Darius the Great about 523 B.C., and on an adjoining hillside they buried their dead.* The influence of this old Christianity on present-day beliefs will be seen later.

It was my privilege to be in Kharga in the spring of 1923 during the middle of the Arabic month Shabaan, when I heard rumours of a festival in connection with a local *Mahmal* to be held on a Sunday, and, after further inquiry, I was invited to be present.† The *mahmal* in Cairo is a purely Mohammedan institution, a litter which, with the Holy Carpet, is carried through the streets preparatory to the latter being sent to Mecca as a covering for the Kaaba, the *mahmal* accompanying it. The

* For some account of Kharga as a centre of Christianity in the third or early fourth century see Scott-Moncrieff "Paganism and Christianity in Egypt," especially Chapter IV.

† Harding King makes mention of a *mahmal* at Kharga (but gives few details) in a paper entitled "Customs, Superstitions, and Songs of the Western Oases" in *The Cairo Scientific Journal*, Vol. VIII (1914). A general account of the sacred litter among the Semites is given by Brenda Z. Seligman in *Sudan Notes and Records*, Vol. I (1918), pp. 268-82.

ceremony had already taken place in Cairo the previous June; moreover, the official Mohammedan Calendar gave no indication of any Moslem Feast at the time of my visit.

Early on the Sunday morning I took a walk round the town. Everyone was wearing his best clothes, and people were going from house to house with salutations and greetings. Then a little later men were seen going into the gardens, from which they emerged with hundreds of palm branches. These they laid lengthwise (east to west) along every tomb in the cemetery; and even little mounds of earth out in the open, which one would never have noticed as being tombs, were all covered in this way.

I then learnt that the Sunday on which was celebrated the cutting of the palm branches and the Procession of the Mahmal was but the first day of an eight-day festival. The following morning, and for five consecutive days, all the youths and young men of the village take part in an extraordinary game. It is played at no other time, and may be peculiar to Kharga. The game is played with hen's eggs, dyed brown, blue, green, etc., with one coloured red, on a specially prepared slightly sloping piece of ground. On the uphill side of this is a specially built mound of sand, carefully smoothed and patted down with the hand. The first player takes the red egg, lays it gently on its side at the top of the sand mound, and lets it roll down on to the prepared pitch. According to the direction in which the pointed end of the egg lies so will it roll, and the object of the player is to hit the red with subsequent eggs rolled down. A "bank" is run by an enterprising youth who acts as croupier, eggs costing two millièmes or one halfpenny. A player who is rash enough to buy five eggs may miss the red every time, and his money goes to the bank; whereas another player may break the bank entirely by a series of hits.

During this week the inhabitants of Kharga are inspired to special culinary efforts. On the Monday a particular species of pancake is made of flour, dates, and sugar, and is eaten religiously by everybody. Thursday is the "Feast of Lentils." These are cooked and eaten in the usual way, but the water in which they have been boiled is taken up to the roof and poured with much ceremony over the walls of the house, each householder repeating at the same time a proverb which, although difficult of translation, may be loosely rendered "Fly away, Death, and be gone." On Thursday evening pastry is made, and on the Friday the wealthier inhabitants spend the day in a house-to-house distribution of these little cakes among the poor. On Saturday the egg-billiard balls are cooked and eaten—a fitting and economical conclusion to the game for the season. Sunday—the last day of the festival—provided yet another delicacy, more subtle than the other simple dishes, in the shape of cooked rice served up in stewed vine leaves.

Every day of this festival has some marked characteristic, and since there is nothing special to eat on the Tuesday the men indulge in blood-letting, which practice is called the "Feast of the Pool of Blood." Wednesday is called the "Feast of the Refreshing," all the inhabitants bathing in water into which has been put a particular sweet-smelling herb, rather resembling mint. Saturday—the last day but one of the festival—besides seeing the conclusion of the billiards game, is remarkable for a curious practice. The feast is called the "Saturday of Light," and is celebrated by both men and women putting black "kohl" round their eyes. As a daily event the use of this cosmetic is confined to women, no self-respecting Egyptian woman presuming to go out without previously making up her eyes; among men it seems to be limited to festive or at least ceremonial occasions.

Kharga village resembles something between a rabbit warren and a pigeon cote. In the old days the marauding Arabs used to gallop up to the town and carry off the inhabitants as slaves and pillage the houses. So as a measure of self-defence the roads were mostly built about two yards wide and six feet high, with houses all

round and overhead, leaving some of the streets in total darkness except for an occasional beam of light. All the streets running through the town, even where not in semi-darkness, are very narrow and tortuous, and it would be impossible for a laden camel to go through them. There is, however, one wide open road encircling the village, from a large open space on the north, all round the houses to the south, and again leading northwards to the same point, and it is this road (the only possible one) which is chosen for the yearly exhibition of the *mahmal*.

About 2 p.m. on the first day of the festival I went to watch the start of the procession from the Mosque of Ain el Dar, which is situated in the north-west corner of the village. Enormous crowds were collecting all along the wide road, and in front of the mosque itself about fifty men and boys had gathered, dressed in their best clothes, waiting for the *mahmal* to be brought out from the mosque. The Kharga women are generally married at the age of ten, after which they remain shut up in their houses, and are scarcely ever seen except as heavily-veiled and muffled figures bolting across the street from one house to another. Consequently on these occasions the women line the roofs of the houses and every vantage point from which they can see without being themselves conspicuous.

The camel chosen for the procession was brought up to the mosque, and after a few minutes the *mahmal* was brought out and put on his back. The *mahmal* itself consists of highly coloured gaudy tapestries made in several pieces and draped over a wooden frame specially constructed to fit a camel's back. On it in white appliqué on a crimson background are worked various designs, including "Allah," the Islamic expression of faith, "There is no God but God," and representations of an object rather resembling a badly drawn cup and saucer.

As the procession prepared to start a band of musicians appeared, headed by a conductor clad in a long white robe tied in with a brilliant green waistband. I noticed a reed pipe, a pair of cymbals, one large drum, two side drums, and two small kettledrums consisting of brass pots like finger bowls, across which goat skin had been stretched. As the procession moved off to the south a group of locally enlisted police went ahead to clear the route, and I cut across the narrow streets to await the *mahmal* on the eastern side of the village. Here I found the route thronged with an excited crowd, and took up my stand at a corner among the multitude. After a few minutes a faint sound was just audible from the distance, the other instruments being drowned in the hollow reverberation of the drums; then, as the procession came nearer, the elusive air of the quavering reed pipe could be heard struggling bravely against the syncopations of the cymbals and the band. The music was truly Eastern. The same regular cadences repeated over and over again for hours at a time seem to penetrate everything, the throb of the mechanical beats thudding on the brain, to the exclusion of external influences and commonplaces.

As the drumming came nearer and grew louder, in some inexplicable way I suddenly felt myself held by the savage appeal of the music. I felt as the crowd around me looked, spellbound. The procession suddenly appeared round a bend in the street, and everyone strained forward to watch, as the camel with its top-heavy load proceeded clumsily along the uneven surface of the road. The tense excitement among the crowd was infectious, increasing as the *mahmal* drew nearer. When it had reached the corner where I was standing the camel was brought to a standstill, and the band played with redoubled energy, while a man emerged from the crowd carrying in his arms a crippled child. The child was lifted up and held for a few seconds under the canopy of the *mahmal* with a silent prayer for his recovery; then, dazed, terrified, and crying pitifully, he was withdrawn and handed back to his father, and the procession moved on. When it had reached the eastern corner of the open space at the north of the village, followed by the entire populace, the procession finally halted, and the *mahmal* was reverently lifted off the camel and

deposited on the ground, with an escort of local notables to keep vigil over it. By the side of this open space ran a stream flowing from one of the wells, and just before sunset the *mahmal* was left in charge of its special escort, while the rest of the crowd went to the stream to carry out their ceremonial washing before the sunset prayer.

After the prayer everybody flocked home, to return after half an hour with food for supper, not only for himself but for one or two hungry mouths besides; and then in the light of the full moon the whole town sat down to supper together, rich and poor, master and servant, in the immediate vicinity of the *mahmal*. Later on in the evening the camel was again saddled up to carry his precious load back to the mosque of Ain el Dar, where it was to remain till the next year.

A senior Egyptian official from the Ministry of Wakfs, whose duty it is to inspect mosques, and to see that the Faith of Islam is properly carried out and taught in the provinces, was sent down to Kharga, and by chance happened to arrive just in time to witness the yearly *mahmal* procession. He was horror-struck at what he saw, and denounced everything that he had seen and heard as Christianity masquerading under the cloak of Islam.

When one learns that the Kharga inhabitants carry out what closely approximates to baptism of infants two days after they are born, and remembers that the Arabic month Shabaan occurs at Easter time (the particular *mahmal* that I saw coincided with the Coptic Palm Sunday), one is forced to the conclusion that he was right, and that the *mahmal* with its eight-day festival is nothing but a Christian Holy Week, in which Palm Sunday, Shrove Tuesday, Easter, and the Last Supper, have become distorted and confused under the outward veneer of Islam.

B. L. AUSTIN KENNETT.

France : Archæology.

Forde.

Megaliths and Metals in Brittany. By C. Daryll Forde.

In two papers published in 1915¹ and 1921², Mr. Perry suggested that the distribution of megalithic monuments in certain regions could best be explained on the hypothesis that the primary motive of settlement was the exploitation of raw materials, and particularly of certain metals, including gold, tin and copper. This view, although criticised and often dismissed on *à priori* assumptions, remains the only working hypothesis of value which has been put forward to account for the apparently bizarre distribution of these monuments in western Europe. In the second of these papers the detailed distribution of megalithic monuments in England and Wales is analysed. Remarkably close correlations between the distribution of megaliths and accessible sources of flint, gold, tin and other substances are shown.

The obvious kinship of the Megalithic Civilisation throughout Western Europe should afford opportunity for the corroboration of this hypothesis. If such localisations hold in England and Wales, similar localisation should be discernable as motives for settlement in the other areas of Megalithic Culture. In order to test the value of the working hypothesis enunciated by Perry, I began, in 1924, to consider its application to one of the most important and extensive of the megalithic areas, the peninsula of Brittany.

The monuments of this region are exceedingly numerous, and the records of distribution are widely scattered, so that a complete analysis of distribution must necessarily take some time. A preliminary survey, however, brings out certain facts. In the first place, Brittany is agriculturally a barren land of old rocks and thin soils, with a damp climate. It was originally heavily forested throughout the greater part of its area³. Such a region has seemingly little from a general point of view to attract dense settlement in early times. The same, of course, applies to many of the other megalith areas of France, *e.g.*, in the departments of Aveyron,

Tarn and Haute Vienne. Fertile agricultural land, such as Schliz postulates as the main factor directing the expansion of the Danubians⁴, was not the main objective of the megalith builders of western Europe.

The distribution of monuments within the peninsula is also uneven. Some areas have a dense concentration, while others are devoid of monuments. Megaliths are found both in some of the more fertile and densely occupied lowlands, as in southern Morbihan and southern Finistère. They occur also in uplands and in barren deserted country, *e.g.*, in the *montagnes d'Arrhès*, so that factors of altitude and fertility alone cannot explain the distribution within the peninsula. The most important megalith area in Brittany lies along a coastal zone, mainly in southern Morbihan, extending from the River Etel to beyond the estuary of the Vilaine. It includes the famous Carnac-Locmariaquer district and the megaliths of the islands in the Gulf of Morbihan and those of Morbraz (the sea to the south of this coast). In this region the correlation that Perry expects is clear. Gold objects have been found in several of the monuments, as, for example, at Mane Lud, Locmariaquer and Rondosse, Carnac. Siret⁵ called attention several years ago to the resources of alluvial gold and tin in the region of the Vilaine estuary and the Gulf of Morbihan. As recently as the eighties of the last century a company was endeavouring to extract gold in commercial quantities from the sands of the island of Houat⁶. Ancient gold workings are also reported from Ste. Anne near Auray⁷; tin lodes which also contain gold occur at Penestin⁸. Moreover, this area contains supplies of rare rocks, which were used extensively for the fabrication of the fine ceremonial greenstone axes placed in the megalithic monuments⁹.

But the main object of this paper is to call attention to some more definite evidence from another part of Brittany. Behind the megalithic zone on the coast of Morbihan lies a belt of country in which megaliths are rare or absent. About 20 kilometres to the north-east, however, on the flanks of the Lizio plateau and the granite ridge to the south, occurs another concentration of megaliths and tumuli. The majority of the tumuli belong presumably to the Bronze Age.¹⁰ The Lizio plateau is a deeply dissected upland of granulite (granite with white mica), a rock with which tin and gold are frequently associated.¹¹ The rivers draining the plateau contain alluvial tin and gold. The tin crystals are often very large, and occasionally reach the size of a small nut.¹² Alluvials have been recorded in stream beds as follows:—In the Oust, near Malestroit; in the Haies valley, near Serent; and in other streams near Lizio, St. Servant, Guehenno, Breman, Castillon, Pendelan, Roc St. André and Quily.¹³ This list is probably far from complete, for this region is apparently the main source of the alluvial gold and tin that is found in the mainstream of the Vilaine. The *massif* contains six groups of tin lodes in quartz veins, which are also goldbearing. The tin lodes of Brittany generally occur as interlaced masses of veins (*stockwerke*) embedded in hard rock, and thin out rapidly. The veins in the Lizio *massif* are, however, more considerable than usual. The largest and best known is that of Villeder (Ville d'air), near the village of Roc St. André. The alluvials are more accessible than the lodes and easier to work on a small scale, so that they would probably have been exploited earlier and more readily; but evidence exists that the lodes were worked at a very early period. The old Breton mineralogist, the Comte de Limur, when a young man, explored at Villeder before modern mining operations had greatly disturbed the ground. Here he found “outcrops of white rock standing up from one to two metres above the general level in a continuous line (of about 100 metres) like a long wall . . . it was an outcrop of *quartz fétide* containing rare traces of tin oxide . . . but the rounded and smoothed outlines (of this outcrop) were not the modifications produced by time or the atmosphere but, visibly, the consequence of blows made long ago to extract as far as possible the spangles of tin disseminated through the rock.

"The traces of these attempts at extraction showed themselves still more certainly in two ditches or trenches running along either side of this wall of quartz, several metres deep, several metres wide, and filled with *débris* of this same quartz broken into small fragments. Interestingly enough, every morsel of tin had been carefully removed, so that it was absolutely impossible to recover a single fragment.

"All these indications evidenced attempts at the exploitation of tin at a time so remote that no memory of it remains. . . . As always, according to the legends of many countries, tradition pretended to identify them as the work of the Romans, just as every artificial mound is decorated with the title 'Camp of Cæsar.' But the Romans knew how to attack the most resistant rocks. We have had many opportunities of admiring their mine workings and galleries in different countries. Cracked and fragile quartz would not have compelled them to scratch in this way at an interesting deposit.

"It is necessary, therefore, in order to account for these immense accumulations of fine *débris* to admit that one was in the presence of an ancient exploitation of tin at the time of the Bronze Age, and that, lacking adequate tools, the miners were limited to a mere scraping of the rock. This supposition was confirmed by the frequent discovery of polished stone axes, notably of fibrolite, in

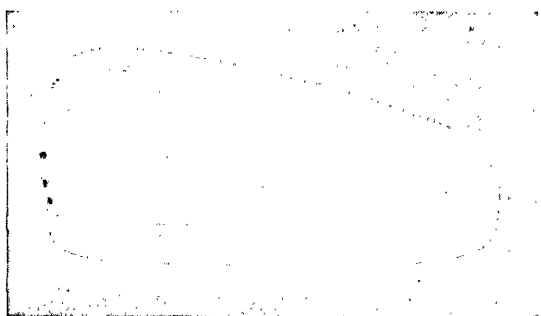


FIG. 1.—FIBROLITE AXE FROM VILLEDER.
Musée de la Société Polymathique du Morbihan. No. 1118.

"the neighbourhood, quite close to these ancient trenches. We obtained from the guardian of the mine equipment a fine polished fibrolite axe of large size (14·8 cms.), found by him while turning up the soil . . . only a metre or so from the outcrop of the large quartz vein. (Fig. 1.)

"The blade of the axe is deeply eroded as if it had served for a long time as a hammer.* Other similar axes have been found under the same conditions, and also several of bronze."¹⁴ Similar open workings are found in other parts of this area. Daubrée¹⁵ describes them as follows:—" . . . 300 metres to the east of the mine of Villeder on the side of the road to Roc St. André, a large excavation is to be seen, other parallel workings occur a kilometer away on the lande of La Hy and still further away near the village of Haute Quily. These excavations, which sometimes have a depth of 3 to 4 metres, contain a large mass of quartz chips, together with fragments of a very compressed pudding-stone (un poudingue très serré) which had long been considered as iron ore; in 1875 it was realised that this was tin-bearing, and that it could be found *in situ* close by." Similar pits can be seen at the present time in the woods between the Château of Villeder and the modern mine workings. Caillaux, who also refers to these excavations, notes groups "on the hills of Ledo, and those which flank the stream flowing to the east of Pourmabon in the commune of Guegon."¹⁶ Daubrée also records the finding of *débris*, from which alluvial metal had been obtained, and tin grains in a meadow between the lande of Hy and Haute Quily. He concludes his description of all these workings by saying: "perhaps it is not without value to add that Druid monuments [*i.e.*, megaliths] are found near by,

* Now in Musée de la Société Polymathique du Morbihan, No. 1,118, see Catalogue 1921, p. 64.

"as well as a large entrenched camp." A gold ornament was found in the second dolmen at Kerallant, near St. Jean Brevelay, in this district.

The sketch map¹⁷ (Fig. 2) shows the distribution of megalithic monuments, tumuli, ancient workings, and streams reported to contain alluvial gold and tin.

Accessible resources of tin and gold occur in two areas of considerable concentration of megalithic monuments in the department of Morbihan. One of them—the Villeder district—has, in addition, ancient workings associated with polished stone implements typical of the megalithic culture. This suggests that the builders of the megaliths exploited gold, tin, and certain rocks, and were attracted primarily to this barren land by its mineral resources and settled chiefly near the scene of their exploitation. Subsidiary factors no doubt played their part in deciding

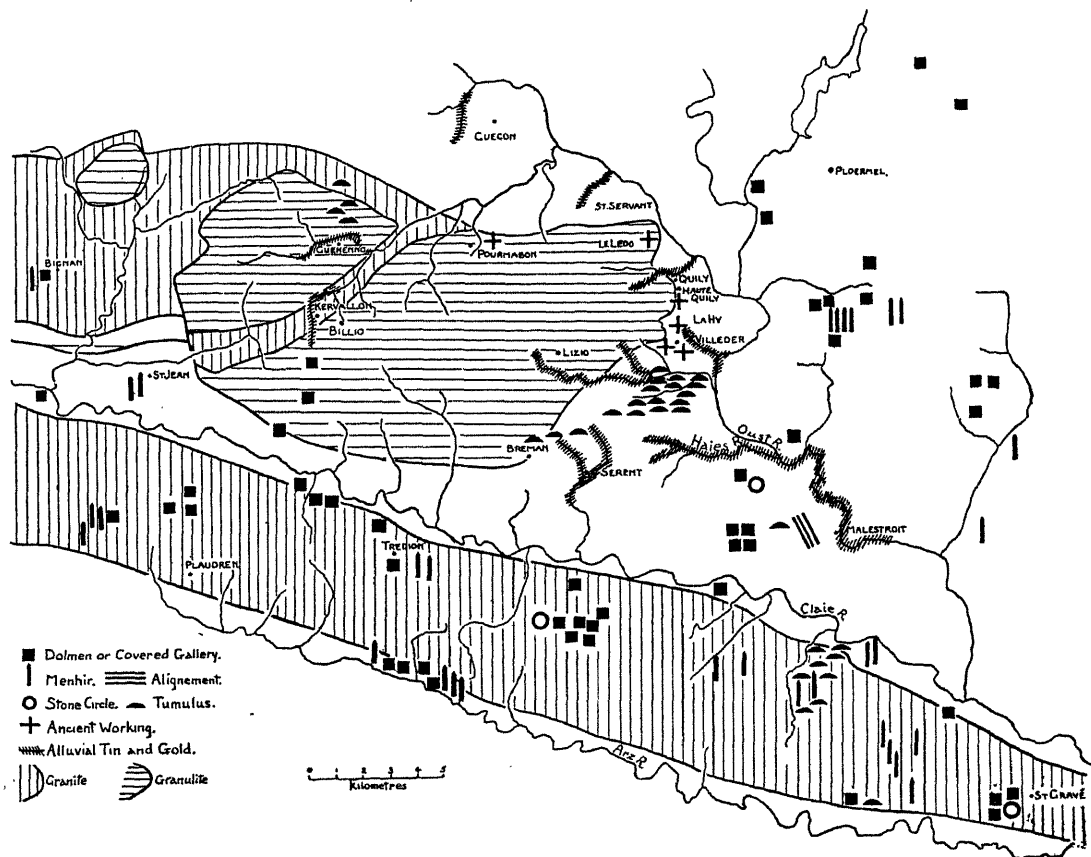


FIG. 2.—SKETCH MAP OF THE LIZIO PLATEAU.

the immediate sites of settlement and in directing the line of subsequent expansion and occupation of the country. This hypothesis attributes to the megalith builders a motive which throughout history has been, perhaps, the greatest stimulus to colonisation. Such a motive, for which there is considerable evidence in Brittany, affords a far stronger driving force, than does the assumption of mere "natural" expansion, to explain the great awakening of civilisation and spread of culture that must have occurred during the times when the building of megaliths was propagated through western Europe.

C. DARYLL FORDE.

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- ¹⁵ Daubrée: *Rev. Arch.*, 1881, Vol. XVI, p. 331.
- ¹⁶ Caillaux, A.: *op. cit.*
- ¹⁷ Based on a map by E. Bassac in the Musée de la Société Polymathique du Morbihan, inventories given by L. Marsille in "De la Lande de Lanvaux à la Lande de St. Méen," *Bull. Soc. Pol. Morb.*, 1909, p. 76 ff, and "Les Monuments ignorés de trois Communes limitrophes," *Bull. Soc. Pol. Morb.*, 1914-16, p. 6 ff, and a personal survey in 1925.

Africa, East: Calendar.

Dundas.

Chagga Time-Reckoning. *By the Hon. Charles Dundas.***88**

More than once I have found, among tribes of East Africa, that certain days are specially observed. Such days may be regarded in a manner as Bantu sabbaths, but they differ from our conception of such days in that they are fixed only as far as the individual is concerned. It may be that it is the custom for a man not to work more than a given number of days on one task, or he must not cease to perform that task on a given day. In Ukamba the simple reason cited was that it is unlucky, for instance, to cease herding cattle on the fifth and seventh day from the date on which such a task was commenced; the seventh day is particularly unlucky since seven days is the incubation period for ordinary witchcraft. I leave it to experts to consider whether the Jewish sabbath might have had any such origin.

Enquiry among the Wachagga elicited the following information, which, briefly put, shows that all days have their significance and influence on man's life and occupations:—

I am not able to explain how it comes that the Wachagga divide their year into twelve months, but I have made independent enquiry in five localities, and in each I have obtained the names of twelve months. There is no reason to suppose that this reckoning is an adaptation of our system, since it is seemingly never known to the young men, many of whom know the European's months and their names, and in fact my informants were always old men.

However this may be, I could find no other system and must suppose that the Chagga year really has twelve months. The names differ considerably in the various localities, but I quote those given in Kibosho, where the elders are said to be accepted by all Wachagga as experts on the whole subject of time reckoning.

The names of these months and their equivalents in our calendar are :—

Oran	-	-	August	Rondoma	-	-	February
Irukume	-	-	September	Ifungade	-	-	March
Materi	-	-	October	Kunyanya	-	-	April
Duuna uta	-	-	November	Kukenda	-	-	May
Duuna niini	-	-	December	Kukumi	-	-	June
Isaadu	-	-	January	Damoi	-	-	July.

The Chagga month has thirty days and is divided into six periods or weeks of five days each.

The first week is called "Saamu ya Ukombe," and its days are: Nussu, Wili, Saadu, Kana, Saamu. The next week is "Saamu ya Shimbata mwiri boo." The days of this week are: Saasadu, Mfungade, Nyanya, Kenda, Kumi.

These two weeks are collectively called "Kumi la Mwiri" or "ten of the month."

The third week is called "Telu." Its days are: Nusu ya Msuo, Wili ya Msuo, Saadu ya Msuo, Kaana ka Msuo, Saanu ya Msuo.

The fourth week is "Maana" and its days are named as in the preceding week, excepting that "Maana" stands in the place of the word "Msuo."

The fifth week is called "Reema," meaning Darkness. The days are named as in the second week.

The sixth week is called "Saanu yeta Mwiri," and its days are named as are those of the first and third weeks.

The month begins with the disappearance of the moon; the name of the first day "Nsu," means "It has passed." Both this and the second day are unlucky; work done on these days brings no profit, children born on the first and second day have no luck and die young. The third day, however, brings fortune, for, as they say, "those who mount to great heights can see the moon." On the fourth day the moon is seen by all and they greet it, saying: "Moon, I see you once more, let then my father's cows bear heifers and those of my mother's brothers bear bulls that they may be slaughtered and I have my share." This is a favourite day for meat feasting. The fifth day is also known as the "Closing," because it terminates the first week. People born on this day are reticent, taciturn, mean and rude, "they do not return greetings."

The sixth day is a fatal one, those born in it die suddenly and their children are short-lived. Hunters, on the other hand, favour this day just because of its fatal influence, which leads game into their traps. It is recalled that the warlike Chief Sina, of Kibosho, was attacked by the Germans on the sixth day of the week; that on account of the day he was minded to surrender, but was dissuaded by others who scorned the white men, and that hence many died on that day. The seventh is the hey-day of all animals, birds, insects and reptiles. They perambulate at large in the bright moonlight, wherefore mothers warn their children against staying out after dark. For human beings it is an extremely unlucky day. The parents of children born on the seventh day die soon after; domesticated animals born on this day are slaughtered soon after they are weaned. The day is also called "Kiwi"—meaning Death—and it was formerly favoured for the fashioning of shafts and handles for weapons; but to this day the Chagga man will not cut timber for honey hives and other peaceful purposes on the seventh day.

The morning of the eighth day is blessed and those born therein are particularly prolific, but the afternoon brings the curse of strife and contention. The great Chief Rindi attacked Chief Sina on the eighth day of the month and accordingly

prevailed in the morning but was defeated in the afternoon. The ninth day is unlucky, but the tenth day is one of good fortune. It is also called the "male day," for boys born on the tenth day grow up to be fearless men and vanquish their enemies in the fight and before the judge. It is a favourite day for the celebration of marriages and sacrifices, as also for occupying new huts.

The eleventh day is unlucky, the twelfth day lucky. Children born on the thirteenth day develop the peculiarity that they are given to straying from their homes; when they marry they are prone to desert their spouses, and they are thievish; in olden days they were very often killed at birth as being too troublesome to bring up. Equally troublesome are children born on the following day; they are mostly sickly and therefore an expense to their parents, while in later life they are treacherous and quarrelsome, particularly so girls, who are incorrigible in this respect. The fifteenth day is so unlucky that nothing is undertaken which can be postponed.

The next five days—that is to say the fourth week—are all extremely lucky and are spoken of as "the days of GOD." The last day of this is known as the "King of Days," and is specially favoured for sacrifices to God. Everything prospers in this week; children born in the Days of God are virtuous, handsome and prolific; their offspring are virtuous and their grandsons beget male children; animals are fertile when born in this week; people plant seed, and many sacrifices are offered.

The fifth week is entirely unlucky, excepting that the last day is favourable to the female sex. Only girls are born on this day and they become virtuous, beautiful and prolific. This day is selected for the initiation of girls.

The last week marks the waning of the moon and its influence is corresponding to that phase. People born in this week never succeed. A man may be brave but he is not lauded for his deeds, he may marry well and have many children but they die young, and he will not reap the fruits of his industry, his stock will fall into the hands of others. Animals born on these days are all such as are dedicated to the spirits, so that they have to be slaughtered, for which reason they are called "Kyandu"—knife. The twenty-seventh day, for instance, is remarkable because people born in it are lean, poor and enjoy but a short life; seed planted on this day may flourish at first, but it soon withers. The twenty-eighth day is also known as "the day of hiding": its children are never recognised; things done on this day are obscure, but for this reason the day had its uses in so far as in former times it was a good day on which to hide from invading enemies, as also for holding secret war councils and making other preparations for war. Sina favoured this day for putting people to death, not only because they would be taken unawares, but also because their fate remained unknown to their friends and possible avengers. The last day of the month is one of confusion. People born on this day are bashful, stupid and muddle-headed and often thievish.

The bane or fortune of each day affects domesticated animals equally as it influences human beings. They are prolific, barren, short- or long-lived, valuable or worthless according to the day on which they are born.

Scarcely less influential than the day is the hour in which a person is born. Thus, boys born at sunrise are favoured of fortune and become leaders of men. For girls the hour is less lucky. Children of both sexes who are born at about 8 a.m. will have long life, but they are mean and unbeloved, for they were born in the hour when the women tie up their loads and go to market—"their souls are tied," it is said. An hour later the women are on their way to market. Children born in that hour are called "travellers." They are restless folk. Between 10 and 11 a.m. the women are busy buying and selling in the market. The children

of that hour are called "sellers," and grow up to be thriftless men and women. A little later the women bind their loads again preparatory to returning home, wherefore the children of this hour have the same traits as those born at 8 a.m.

From noon to about 3 p.m. the sun is fierce and drives all men into the shade; children born at this time of day are blessed, but short lived. Those born between 3 p.m. and sunset grow up to be clever and industrious, for they were born in the hours when the women are busy in their huts with their domestic duties. Lastly, children born in the night have luck, particularly in escaping death and injury, for they came into the world in the hours when the huts were closed and secure.

The belief in the influence of the day was sufficiently firm to weigh in the councils and undertakings of powerful and ruthless chiefs but a generation ago, and it is hardly possible that the little sophisticated native of to-day should have become indifferent to such supposed influences. The inexplicable actions of natives are so often due to such superstitions that one may well suppose that the day and hour of birth or the fortune or ill luck of a day frequently play a part in the most practical affairs of life in Africa. Reluctance to undertake a particular task, the sudden disappearance of individuals from their contracted service which so often puzzles the European employer and makes him despair of ever making a reliable labourer of the native, may not infrequently be due to these beliefs as well as to a host of other superstitions that constantly demand the native's attention to things spiritual and mysterious. We laugh at them, but to the native they are a real burden, a malady which no doubt can be cured in time, but if we are to effect the cure with a superior wisdom we must at least know what the malady is or we shall grope in the dark as much as the native. But as time goes on, while the superstition remains, the native is less and less able to give us its clue because he does not understand it himself, and so the new generation is less able than were their fathers to assist us with a true understanding of the difficulties they had to contend with. This is my plea for recording such information as I have, though it be sadly incomplete in detail. Time goes fast in Africa and with it much knowledge goes by, which one day we may sorely need; it will be like the twenty-eighth day for both black and white, a day of obscurity, good only for hiding.

CHARLES DUNDAS.

Papua: Folklore.

Riley.

The Origin of Mankind. *By Leo Austen.*

When I was stationed at Abau in the Mailu district of Papua I obtained information regarding the various tribes of the inland villages between the coast behind Abau and the main range. The people continually referred to the Kina people. Not knowing this tribe, I made further enquiries, and found that the Kina were the alleged spirit people from whom the existing tribes are descended, and I obtained the following legend from two very old men of the Mawabula tribe. Mr. Saville of Mailu informs me that the Mailu tribes also have a legend of the Sina, which he is about to publish. It will be interesting to compare the two tales, as they may indicate a common ancestry of the bush and salt-water people.

Among the foothills of the main range of Papua, about two days' journey from Mawabula village, and lying between this village and Ameara mountain, is a hill called Babaibo. This hill is not very high compared with those north of it, nearer the main range. Long, long ago, before there were any people living on the earth, a tree called Isoa grew on the top of Babaibo. This tree was an enormous one of immense girth, whose roots reached down into the bowels of the earth. (One can easily imagine its size, by looking at the hole it has left on Babaibo. This hole goes down, down into the blackness of night. We two have

seen it, for one day we crept softly up to the hole and gazed down into the depths below, but we became frightened and ran quickly away.) In Isoa lived two spirit people (Kina), whose names were Kau and Keboro. Kau was a spirit man, and Keboro was a spirit woman. These two had innumerable children, during the ages that passed away, and eventually the space inside the tree was full of spirit children. One day, when the children were playing, they kept hitting the side of the tree. Unfortunately, they pierced a tiny hole in the side of it, through which a ray of light flowed in. Keboro, seeing this brightness, was very curious to see from where it came, and on peeping through the tiny hole saw the wonderful land and bright blue sky outside. She told Kau, but he became very wrath, and swore roundly at the carelessness of his children. But Keboro, having looked on the world outside, longed to change her confined abode; and by and by she persuaded Kau to leave their dismal dwelling place. Kau and Keboro and all the children then climbed up inside the tree trunk and broke open the top of the tree. Great was the joy of all the spirit children to see the golden sunlight, and the wonderful colourings of the world above. Eagerly they clambered out of their old home, now become so dark and dreary in comparison with their new world. Kau told all his children that they must now leave him, but, before they left, he divided them into groups, and to each group he taught a different language, and all the primitive arts and crafts with which he was familiar. Then he portioned out the land, and gave each group its share. To one group he taught the Bawake language (to which group we belong), and sent them over the main range to the watershed of the Musa river. There they formed four villages on the hills of Dowebi, Iau, Bogoru, and Ko oubi. These were the places on which the present Ukaudi, Mawabula, Kuroudi, and Keveri tribes originally dwelt. On the Musa side of the range, the Anime and Abie-speaking groups were also sent, but they settled to the west of Ameara mountain. To those peoples who were to live near the salt water, Kau gave orders to build large houses, for they would increase rapidly; but he advised those who had been allotted inland portions, to make only small houses, as they would be continually warring on one another, and so many would be killed that their villages would remain small.

As the ages went on, Kau and Keboro died, and the spirit people (Kina) gave birth to a new race of people—the Emiaga or true people of to-day—who were not invisible, but composed of real flesh and blood. From these are we descended.

This is the story of our people, as told to us by our fathers and mothers.

LEO AUSTEN.

Africa, South: Currency.

Lindblom.

Copper Rod "Currency" from Palabora, N. Transvaal. By *Dr. Gerhard Lindblom.*

90

In MAN, 1918, 65, Dr. Haddon described and illustrated an object of copper, peculiar in form, from Palabora, N. Transvaal, supposed to be a form of currency, and H. D. Hemsworth, in No. 65 of the same year of issue, has added informing supplements to Dr. Haddon's article. As the object in question appears to be but seldom met with in European museums, I beg to call attention to the fact that at the Stockholm Ethnographical Museum there is one, provided with statements that coincide both with those of Haddon and those of Hemsworth.

The specimen (Fig. 1) in the Stockholm Museum (marked 07·11) was a gift to the Museum in 1907 from a Captain J. A. Larsson, Leydsdoorp, Transvaal, unknown to the writer of these lines. It is 49·8 cm. in length, the diameter of the rod being 11–13 mm. (therefore of the same size as the two described by Haddon) and is provided with six rootlike bars (each 20 mm. in length), three on each side of the

broad, flattened projection. Their dimensions are $6.2 \times 5.9 \times 2.75$ cm. The weight of this specimen is 900 grammes. The surface is comparatively smooth, with the exception of the end of the flattened cone, where, without doubt, the mouth of the casting mould was. Larsson bought this specimen from a native "of mixed negro race, named M'zeke, in whose family it had been transmitted for some length of time. According to information given it is supposed to have an admixture of gold.* According to tradition, this currency is very ancient, but has for some length of time fallen into disuse, and therefore specimens are extremely rare. In days of yore, according to tradition, a wife could be bought for five of these copper rods, while in exchange for a single rod, two head of cattle were given. This currency is derived from Palabora (the district lying east of Leysdoorp), where they point out an ancient closed mine, Lulukop, whence in former days the ore for the currency was obtained. There, moreover, clay-pipes a foot or so in length have been unearthed, which have been used in the process of smelting, as also knocking-stones, etc. etc."

Furthermore let me add that a similar specimen is illustrated in *Zeitschrift für Ethnologie*, 1893, p. 320. This specimen (Fig. 2) is from "den Knopfneussen in Bolubedu (am Limpopo)" so from the Makoapa people, nicknamed "Knopfnasen" by the Boers. The length of the rod is 49 cm. with a diameter of 13 mm. The cone at base measures 4 and 8.5 cm.

FIG. 1.
(THE STOCKHOLM
MUSEUM).

Of the four specimens treated here, three are from Palabora, the Basuto population of which were influenced both as regards culture and in no small degree in the domain of the copper industry by their neighbours of the interesting Balemba tribe. The fourth specimen is from rather farther north. However, if we remember that the dimensions of the rod are the same in all four specimens it is surely not too much to infer that they all, at any rate anent the type, are originally from the Balemba. The specimen from the Makoapa has perhaps found its way to them as barter ("currency"). Moreover, Balemba men, working as copper-smiths, now and again live beyond their district. According to Schlömann, the missionary (*Zeitschrift für Ethnologie*, 1894, p. 69), the Balemba tribe are immigrants into their district and have probably come from somewhere north of the Zambesi. In many respects they differ from their neighbours (the Bawemba and the Matabele mixed with the Basuto). "Ihre religiöse Gebräuche, ihre Physiologie und Charakteranlage sowie ihre gewerbliche Bethätigung sind wesentlich verschieden von derjenigen aller anderen südostafrikanischen Stämme," Schlömann declares, and cites many instances in proof thereof. Among others, that they were far superior to their neighbours in working copper and drawing it to wire. "The Balemba have been the true pioneers of civilisation among the Ba-Suto, who were then in a very primitive condition," Junod states ("The Balemba of the Zoutpansberg (Trans-

FIG. 2.
(AFTER *Zeitschrift
für Ethnologie*, 1893).

* Chemical analysis of the specimen proved that this is not the case: "The sample consists of copper. Neither gold, silver, tin or zinc were present."

"vaal)." *Folklore*, Vol. XIX, p. 280) who also urges their skill in working copper and iron. According to him they introduced the metal industry among the Ba-Suto.

Both Schlömann and Junod, therefore, agree in considering the Balemba to stand far above their neighbours, and, according to these two authors, they must once have resided in some place where they were subjected to Semitic (or Mahometan) influence. Carl Mauch, the rediscoverer of the Zimbabwe ruins, paid special attention to the Balemba, and, though he only devotes a few lines to them, he states that both physically and culturally they differ from their neighbours. They remind one of the "Israelitic types" he declares, and have their peculiar customs, etc. etc. The Balemba he mentions appear to have lived between Limpopo and the Zambesi. (C. Mauch, "Reisen im Inneren v. Süd-Afrika, 1865-72," *Petermann's Geog. Mitteilungen*, Erg., H. 37. Gotha, 1874). According to Schlömann they obtained the material for working copper "meistens von ihren Landsleuten im Bawenda Lande (Northern Transvaal) und sie behaupten, jene bezögen es heute noch aus der Gegend von Zimbabwe, zwischen Limpopo und Zambesi. Die von dorthier bezogenen Kupferbarren haben oft die eigenthümlichsten Formen. Meistens stellen sie sich als trichterförmige Metallklumpen dar, die in einen 1/2 bis 3/4 m. langen Stab auslaufen."*

It appears to me of importance that the Balemba (the only people in South Africa that appear to have made or traded with these copper objects) are immigrants from the north (the sole specimens anything like them that I know of are the copper castings from the Magaliesberg Bakuena, figured by Stow) ("The Native Races of South Africa," p. 518) and these people also came from the north.

With regard to the use of the specimens mentioned, their appearance as "currency" is probably only a secondary step, caused solely by the value of the metal. At first they were doubtless semi-manufactured goods for further utilisation for various purposes. What furthermore concerns their form, the specimens without the root-like bars, about which both Schlömann and Hemsworth make mention, appear to have been the most usual and directly caused by the process of casting. This probably was carried on in such manner (there are similar statements from other South African peoples) that a stick with a lump of clay or wax at one end was passed in a slant through a heap of packed, moist sand and the metal was cast in the mould thus formed. If it were desired to acquire the root-like bars, a stick was stuck down the walls of the mould making holes.† A specialist in Stockholm, H. Bergman, who undertakes artistic casting, confirms the opinion that the specimen Fig. 1 has been cast in a single piece, and that the casting was accomplished in manner mentioned. The root-like bars are, therefore, no necessary consequence of the casting, but an extra addition thereto. The circumstance that there are specimens without appendages furthermore seems to speak for this fact. Contrary to the opinion of Hemsworth, I also consider that those specimens provided therewith possess some special meaning and that the numbers of the appendages have some significance. There is the unwillingness of the natives to dispose of these specimens to favour this theory.

Here the word Zimbabwe has already been mentioned, and there is a temptation to place the Balemba "currency" of copper rods in connection with the as yet undetermined origin of the Zimbabwe culture. It is true that copper articles have been unearthed among the ruins of Zimbabwe, but nothing corresponding to the subject of this paper, so far as I am aware. For the present, at least, I must leave both their signification and their origin unexplained, much to my regret. Their

* Italicised by the author.

† The opinion advanced in *Zeitschrift für Ethnologie*, 1893, that the appendages had arisen by air-channels being made that had subsequently been filled by the fluid mass can scarcely be correct. This opinion has probably arisen owing to the specimen in question (Fig. 2 in this paper) having simply two such appendages.

origin, however, is doubtless connected with the past history of the Balembas, as yet unknown to us. According to their very ancient traditions, as Schlömann tells us, they have once lived by the "Loathe River." "But Loathe is an abbreviation of Leoathe, and signifies sea." Perhaps in far-off times they have once lived by the Indian Ocean and there come in contact with Arabs or Persians. It is not incredible that either in Arabia or Persia there may be something corresponding to these peculiar appendages described above. Concerning this question it would be of importance to have a summary of the entire material culture of the Balembas, as it might possibly provide some clues.

GERHARD LINDBLOM.

REVIEWS.

Archæology : Stone Age.

British Museum.

A Guide to Antiquities of the Stone Age in the Department of British and Medieval Antiquities. British Museum. Third Edition. London: British Museum, 1926. Pp. 204. Price 2s. 6d.

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This issue supplies a text-book of high importance and excellence. It must be borne in mind that the antiquities of the Stone Age housed in the British Museum constitute but an incomplete collection, and since the Guide is restricted to interpreting the specimens in the Museum, readers must not expect that full treatment of the subject which the learning of the distinguished author could otherwise have given. This edition contains much new material, and the views expressed are less conservative than those of its predecessors, a change largely due to cumulative adverse evidence. The pick and the shovel are indeed grave diggers. Noteworthy is the fact that the basis of the chronological scheme now adopted is, to a very great extent, the result of the work of one of our countrymen, Mr. Reid Moir, whose discoveries relating to Pre-Crag Man are producing creeping paralysis in the ranks of the opposing school of thought. Certain of this school, it may be remembered, have diagnosed as modern builders' refuse the "working site" discovered by Mr. Moir upon the beach at Cromer, and relegated by him to Pre-Chelles times. That the Cromer beach industry will be proved contemporaneous and identical with the industry brought to light by Mr. Hazzledine Warren at Clacton-on-Sea, and termed by him Mesvinian, is a strong probability.

Some of the interpretations in the Guide disclose the influence of those archæologists who desire a Palæolithic origin for Grimes Graves and who postulate that implements similar in form are typical only of one culture period. As an example, in the treatment of the problems arising from the discoveries at Grimes Graves, the writer has attempted to serve two masters, since he allots certain of the finds to the Palæolithic and the remainder to the Neolithic. Now this arrangement may be useful in explaining the occurrence of hand-axes, Levallois flakes, and tortoise-cores in the workings, but is it justifiable? The tortoise-core industry was an extravagant practice and one that could only be adopted to its full extent in areas where large blocks of suitable flint were available. Flint of poor quality resulted in imperfect Levallois flakes. The blocks mined at Grimes Graves were not only plentiful but their quality admirably suited the requirements of the prehistoric knapper. It must be remembered that the method of detaching flakes from tortoise-cores persisted from St. Acheul to well into the Bronze Age, and evidences of the survival of the more serviceable types of implements through successive culture periods are accumulating from day to day. Consequently, there is no reason why the hand-axes, Levallois flakes, and tortoise-cores of Grimes Graves should have been classed as Palæolithic.

Most archæologists favour a Neolithic origin for Grimes Graves, but it seems probable that they were first worked during the closing phase of the Epipalæolithic period. It may also be considered that the fundamental principle which at present governs the dating of such prehistoric remains is ill-founded. Where geological

evidence is wanting, the date of an industry should be fixed by examining all the products of the industry and by adopting as a basis the major implement, latest in type, which occurs in quantity upon the site in question. Thus to attempt to date Grimes Graves by its hand-axes, Levallois flakes, and tortoise-cores would be erroneous. The celt is the dating factor of the industry.

Dealing with Aurignacian finds in France, on page 130 there is the statement—"Though rare in England, the graving tool is abundant in the French caves." This information is insufficient, for, although it is true that Palæolithic gravers are rare, there have been found in England, hundreds, perhaps thousands, of gravers upon Epipalæolithic and Neolithic sites.

On page 100 a reference is made to the finding at Abor Low of kite-shaped and pointed oval arrowheads superimposed on the barbed and tanged variety, which, we read, is theoretically later. In view of Mr. Hazzledine Warren's important stratified finds at Walton-on-Naze, supplemented elsewhere by other research workers, it must be admitted that the barbed and tanged arrowhead made its appearance in England at some time prior to the coming of the Beaker Folk, with the result, therefore, that the sequence recorded at Abor Low is consistent with proved facts and need not be regarded as unorthodox. Further, this contributes to the evidence of the survival of types, since barbed and tanged arrowheads are a feature of Bronze Age interments.

For the rest, the publication is admirably written and provides a most interesting and instructive Guide. The author, Mr. Reginald Smith, is to be congratulated for this, his latest, service to archæologists.

Both students and others will agree with the regrets expressed in the Preface by the Keeper of the Department concerning the Sturge Collection. Delay in issuing a hand-book to the exhibits is readily understood, but that visitors to the Museum should still be precluded from seeing this valuable and unique collection which, as a gift, became National property eight years ago, is a matter that invites prompt action by the Authorities

J. P. T. BURCHELL

India: Religion.

Religion and Folklore of Northern India. By William Crooke, C.I.E., prepared for the Press by R. E. Enthoven, C.I.E. Oxford University Press: Humphrey Milford, 1926. Pp. 471. Price 21s.

The late Dr. William Crooke's standard work, *Popular Religion and Folklore of Northern India*, published in 1896, has long been out of print. The present edition, which has been entirely rewritten, includes a vast quantity of additional information collected by Dr. Crooke after his retirement from the Public Service in India. He was, unfortunately, not spared to see the work through the Press himself; but this task has been wisely entrusted to Mr. Enthoven, a fellow-member of the Indian Civil Service, whose own researches into the religion, folklore, and social grouping of the people of Western India have rendered him an expert on the subject of popular Indian customs and superstition. The scheme of the work follows the *questionnaire* drawn up years ago by the author, when he was collecting materials for his original work; and the main

Crooke.

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conclusion at which he arrives—and none who have served in the rural districts of India will deny the justice of his finding—is that "the religion of the Indian peasant is largely based on a feeling of fear which it has never wholly shaken off." That the religious ideas and cults of the Indian villager are liable to change, Dr. Crooke admits; and he indicates shadowy signs of new ideas engendered by political and material changes in his environment or by contact with the ideals of other faiths. But he adds that "in the present intellectual and economic stage of peasant life there seems little chance of the decay of this rural worship." The peasant, indeed still clings to a religion and ritual based on the need of pacifying or controlling the myriad forms of evil to which he believes himself to be exposed; and, as Dr. Crooke suggests, the sole fate ultimately open to this universal form of local and village worship will be more or less complete absorption in that medley of magic and metaphysics which is broadly styled Hinduism.

The opening chapters deal with Nature gods, village godlings, and the worship

accorded to them; and at the end of Chapter III is an interesting tale of Momiāi, which Mr. Enthoven in his editorial preface confirms by a parallel incident which occurred in Bombay during the first plague epidemic of 1896. The chapter on disease godlings contains much interesting matter concerning rural methods of treating leprosy, hydrophobia, small-pox, cholera, and the diseases caused by the malignant spirit who personifies the Hindu cremation-ground; and instances are given of the scape-goat rite and other forms of sin-transference. Under the cult of ancestors the custom of *Sāti* is explained and illustrated, and allusion is made to the worship still offered at Souir in Poona to the spirit of Colonel Wallace, to whose grave the villagers annually bring the first-fruits of their fields. Unconscious humour sometimes characterises this worship of departed Europeans, as in the cases of Captain Cole, killed at Travancore in 1809, who is propitiated by offerings of wine and cigars, and of "a bibulous official, who died at Muzaffarnagar," whose spirit is appeased with gifts of beer and whisky.

In the course of the chapter on the malevolent dead and demons, Dr. Crooke refers to the South Indian custom of wedding the corpse of a bachelor to a living girl, and suggests that the revolting proceeding described by some writers is based on a misunderstanding. This is probably correct. At any rate no trace exists to-day of the similar rite, described by the Abbé Dubois, of marrying a living man with a dead girl. India offers evidence of the *cowade*, which is explained as intended to express the close relationship between husband, wife and child, to prevent danger to the two latter from any action of the first, and to serve as a recognition of the father's parentage. Various methods of conciliating the ghosts of the dead are described, the figure of the *Bhagat* or medium being common throughout India, and instances are given in the chapter on Fertility rites of the belief in human blood as a cure for sterility. It may be mentioned that the annual provincial police reports give several later and more recent instances of this class of murder than that mentioned on page 242. The marriage of girls to gods, which is common in the Deccan and Madras, is described as a charm to promote vegetation and fertility, and many illustrations are given of agricultural rites and festivals designed to ensure the fertility of cattle.

The remainder of the work is devoted to such subjects as the evil eye, the worship of material objects, fire, the worship of animals, serpents and trees, and the black

art and witchcraft. Dr. Crooke's work will be valuable not only to the academic student of primitive religion and customs, but also to all young Englishmen who choose a career in the Indian Public Service. The evidence, so carefully collected and set forth in these pages, unquestionably justifies the author's final opinion that any relaxation of the reign of British law in India would lead to outbreaks of fanaticism and a revival of the grosser forms of superstition described in the course of the work. S. M. E.

Africa, South: Ethnography. Brown. *Among the Bechuanas*. By J. Tom Brown. Introduction by A. R. Radcliffe-Brown. Map and photographs. London: Seeley, Service & Co., 1926. Pp. 272. **93**

The author lived as a missionary for thirty-nine years among the Bechuana; he died before his manuscript was printed. The title is a curious one. It is strange that, although the Bechuana have been known longer than almost any other South African tribe, we have had to wait until now for a monograph describing their customs and beliefs. This is by no means a complete account. It is written in the style of a man who has dwelt so long abroad, and grown so accustomed to speaking and thinking in another language, that he has forgotten how to write English. It is somewhat amorphous. But it contains a considerable amount of very useful material. The latter part of the book sets out the history of the numerous Bechuana tribes—27 are enumerated. Mr. Brown lived mostly, we believe, among the Batlapi; he does not make it sufficiently clear to what extent his descriptions apply to the northern tribes. He believes that the name is derived from *cwa*, "to come out"; *Bacwana* would mean "the separatists"—he does not explain how *Ba-* has become *Be-*. Mr. Brown has much to say about the *seboko*, or "totem." Tribal kinship, he says, "comes through 'the father' when the marriage has taken place according to accepted custom; the father's 'totem' is the 'totem' of the children. But the maternal uncle (*maloma*) has most authority over the children. 'There seems to be no law, or taboo, 'forbidding the marriage of the closest relatives. . . . Cross-cousins have certain 'claims upon each other, the male in 'each case having a prior claim to the 'hand of the female in marriage.' The most informative chapters are those dealing with the initiation ceremonies—here a paper contributed by Mr. Brown to the *Journal of the R.A.I.* is incorporated in a

revised form—and the religious beliefs and practices. It is interesting to compare what Mr. Brown says with Robert Moffat's assertion that the Bechuana had no religion. One wonders how much the beliefs have been affected by the teaching of the missionaries. Mr. Brown says that the *badimo* are sometimes called *badimona*; has the word and the idea taken any colouring from *bademona*—the transliteration by the missionaries of *daimon*? In his chapter on "Priestcraft" he does not differentiate sufficiently the *liṇaka* (whom he calls "professors of witchcraft") from the *baloi* (witches). Mr. Brown is rather obsessed with a possible connection of Bechuana practices and beliefs with "Egyptian" and Semitic; he is even inclined to see some significance in the resemblance of *Bile* to *Abel*, and to suggest that the Bechuana tale of *Bile* is "just a version of the Cain and Abel story." Apart from a few such aberrations, his treatment is objective. He records a few folk-tales, including a version of the legend of the Origin of Death, according to which the two messengers, Chameleon and Lizard, are sent, not by the Creator to men, but by men to the Creator. There are still many things we want to know about the Bechuana; we should have welcomed, for example, a careful study of the tribal system among the Bamangwato.

E. W. S.

Sociology.

Gupta.

The Evolution of Law, by Nares Chandra Sen Gupta, M.A. Calcutta University Press, 1925; pp. 183; 9 in. × 6 in. **94**

This concise and clearly-written work is described by the author, who is Professor of Law at the University of Dacca, as "an introductory exposition of historico-comparative jurisprudence based on 'up-to-date researches,'" and represents the gist of lectures delivered to students at the university above named between 1922 and 1924. Dr. Sen Gupta has evidently read widely, and in the course of his arguments gives many references to the primitive legal ideas and practices of other countries; but he—perhaps naturally—discusses the growth of Indian legal ideas and systems at rather more length, as a protracted study of the ancient law of his motherland has "revealed hitherto unrecognised truths founded upon evidence which is not often available in the ordinary text-books of Hindu Law." After paying a tribute to the value of modern anthropological and sociological research for the study of ancient legal institutions, and

emphasising the importance of magic in the evolution of law, Dr. Sen Gupta discusses the organisation of the ancient family, patriarchal, matriarchal and totemic, and points out that kinship in modern civilised society is a synthesis of the ancient patrilinear and matrilinear forms of relationship, engendered by the gradual disappearance of the old proprietary basis of the family and the consequent acceptance of kinship by blood-relationship in supersession of a kinship based on the authority of the father. India provides a curious example of the transition from agnatic to cognatic kinship in the institution of the *putrika* or appointed daughter, who by a legal fiction was regarded as a son.

The chapter on marriage affords an opportunity of reviewing the various existent theories of the origin of exogamy and endogamy, and of explaining the social tendencies which underlie the custom of marriage before puberty—a problem which has always exercised the minds of Hindu social reformers. The author suggests in his chapter on sonship that natural paternity had been accepted and established by the Aryans before their dispersion and migration, and that the recognition of fictitious paternity, or the transition from procreative to adoptive paternity, was a later process, of which the gradual stages are illustrated by the history of the evolution of secondary sons in Ancient India.

Likewise, in discussing the growth of judicial procedure and the connection between the social law of crimes and the religious conception of sin and penance, he asserts that a wholesome criminal jurisprudence was established in India much earlier than in other lands, owing to the close association of the Raja or ruler with religion. India also offers evidence of the very slow development of a rational law of evidence, superseding the primitive appeal for guidance to the gods in the form of ordeal, judicial combat, (as described in the *Mahabharata*) and oaths. In reference to the law of property Dr. Sen Gupta rightly points out that Maine's theory of property being owned jointly by the whole village has been exploded by later researches in the *ryotwari* villages of India, which indicate that in the most primitive age land was owned by families jointly and not by the whole village. Chapters on the law of contract, on the law of descent and inheritance, and on testamentary succession, in which he explains the reasons underlying the non-development of testaments in India, bring this suggestive treatise to a close. There is an interesting appendix on the Hindu joint family, which indicates the

possible influence exercised upon Vedic Aryan ideas by contact with Dravidian matriarchal societies of the type common in Malabar. S. M. E.

Burma: Linguistic. Naylor.

Handbook of the Chin Language (Siyin Dialect). By L. B. Naylor. 95
Printed by the Superintendent of Government Printing, Burma, 1925.

Mr. Naylor's little book on the unwritten language of the Siyins, a dialect of the Chin language that is spoken in the northern part of the Chin Hills tract in Burma, is timely, as the only other work on this dialect is a small vocabulary by Captain Rundall which was published shortly after the annexation of Burma and is now out of print.

Mr. Naylor's book is divided into three sections, Grammatical Notes, Sentences, and an English-Siyin vocabulary containing about 2,500 words.

It is a pity that there is not also a Siyin-English Vocabulary and an Index.

The book will prove useful to officials serving in the Chin Hills and to officers of Indian Regiments in which there are Chin units. W. A. HERTZ.

Greece: Ethnography. Rose.

Primitive Culture in Greece. By H. J. Rose, Professor of Latin, University College of Wales. Pp. 245, London; Methuen, 1925. Price 7s. 6d. 96

The primitive culture Professor Rose is concerned with is not that unearthed by the archæologist's spade, but that laid bare by the comparative methods of ethnology. He enumerates a number of curious primitive traits in the culture of Ancient Greece and adduces comparisons from the institutions and beliefs of modern barbarians and savages. It is noteworthy that he excludes from the list the alleged traces of "matriarchy" and "Fraserian kingship," and gives very convincing grounds for so doing. With such supposed survivals duly eliminated the really primitive traits persisting into classical times turn out to be surprisingly few—far fewer indeed than are to be found in mediæval Europe or even contemporary France and Ireland. The real survivals that Professor Rose discovers are none the less of considerable interest to the anthropologist, while his anthropological parallels will be even more useful to the average lover of Greece. The classical evidence is very fully stated in a manner intelligible even to those who are not classical scholars. On the other hand, the scope of the work obliges the author to take up a positive attitude on points, such as the sexual ignorance of the Arunta

and the burial rites of lower savages, which are still open. V. G. C.

Austria: Archaeology. Mahr.

Das Hallstätt. By Doctor A. Mahr. Pp. 67. Vienna: Oesterreichischer Bundesverlag für Unterricht, Wissenschaft und Kunst. 1925. 97

Publications about the site which has given its name to the earliest Iron Age are few and far between. Doctor Mahr's catalogue of the Museum in Hallstatt appeared in 1914, and a posthumous inventory of the Hallstatt graves in the Vienna Museum by Professor Hørnes in 1921. Apart from these there are hardly any modern works of importance, and even the two just mentioned only deal with certain aspects of the subject. "Pre-historic Hallstatt," therefore, supplies a real need.

Doctor Mahr is probably the chief living authority on this particular site, and the book itself, though intended as a guide and written on short, popular lines, is of a more important nature than the modesty of the author would lead us to expect.

After a certain amount of introductory material, and an account of the Stone and Bronze Age antiquities found in the neighbourhood, we come to the Hallstatt Period proper. The work is here divided into three parts, which deal with the Cemetery, the Salt Mine and the Settlement. In his account of the former, Doctor Mahr throws an interesting light upon the old, happy days of the Austrian monarchy (p. 23_{2a}): cremation graves 505-507, the richest yielded by the cemetery, all chanced to be discovered on the day that the Emperor, his gracious Consort, and their suite, visited the burial-ground. This is worthy of mention: archæologists of the future might build theories on the richness of these graves, only to find that their arguments rested on insecure premises.

The account of the mine, though short, is very valuable. It seems strange that the salt mine on the Dürrenberg near Hallein, a site by no means as important as this one, should have been the subject of two excellent works by Kyrle, while that of Hallstatt has been so neglected. But perhaps, interest in the mine has been somewhat impaired by the fame of the cemetery.

With regard to the settlement, little as yet is known. The most important find in this respect is part of a log-house, brought to light by Szombathy, some forty years ago. The actual site of the village is yet to be discovered. It will be interesting to see if Doctor Mahr's prophesy as to its whereabouts proves to be correct (p. 34).

The account of ancient Hallstatt is continued into Roman times.

The Retrospect and Summary are of considerable interest. Perhaps the most important contribution is to be found in the section dealing with the effect of the alteration in climate (there was a sudden change for the worse during the ninth century B.C.) on the old salt industry (p. 46). Mahr, I think rightly, differs from Gams and Nordhagen, who ascribe the abandoning of mining operations to a pause following upon a sudden, catastrophic flooding of the pits. This somewhat dramatic view is hardly based on sufficient evidence. Doctor Mahr contends that owing to conditions of general moisture, and a consequent appearance of new and considerable salt-springs on the mountains, the inhabitants of Hallstatt and Dürrenberg gave up mining—probably gradually—and took to the easier method of extracting salt from these springs.

A detailed comprehensive account of Hallstatt is yet to be published. But this excellent little book helps us in bridging the gap at all events for the present, and leaves us with the hope that when the classical work on Hallstatt is written, it will come from the pen of Doctor Mahr. J. M. N.

Germany : Archæology.

Kraft.

Die Kultur der Bronzezeit in Süddeutschland, auf Grunde der Funde in Württemberg untersucht von Georg Kraft. Benno Filser, Augsburg, 1926. Pp. 153+xli plates and 6 maps. **98**

The generalisation of Montelius's typological schemes to apply to wide areas and unrelated cultures has hitherto fettered the progress of research into the development of the Central European Bronze Age. The book before us has many merits but not the least is that it marks a step towards a breach with the orthodox application—or rather misapplication—of Montelius's principles. We may firstly compliment Dr. Kraft on admitting the survival of at least one group of people living in a "neolithic" stage, while other folk in their vicinity were using bronze. The people in question were the makers of corded ware who constituted, as our author has conclusively proved, an important element in the tumulus bronze age culture of south-west Germany. Still more revolutionary and convincing is Kraft's division of the "Tumulus Bronze Age" itself. He has carefully compared and analysed the material from Württemberg and especially the grave goods from the hundreds of barrows. The result of this minute and painstaking study is of far more than local importance. It shows in effect that the

tumulus culture lasted throughout the period of the Late Bronze Age urnfields, and probably well into the Iron Age (Reinecke's Hallstatt A). Kraft has brought down a whole series of types, especially those current along the Danube valley, from the earlier half of the Middle Bronze Age (Reinecke B) to the Late Bronze Age (Reinecke D). Our author has confined his attention to the material from his allotted area, but a fresh survey of the Bavarian, Hungarian and Italian evidence in the light of Kraft's conclusions has convinced the reviewer that he is eminently justified. Indeed this exact analysis of a small local group enables us to see the whole development of the European Bronze Age in a new light which dispels the shadows of many perplexities. This fact and the many excellent illustrations make the book a standard work which every archæologist must possess. V. G. C.

Balkans : Archæology.

Casson.

Macedonia, Thrace and Illyria.

By Stanley Casson, M.A. Oxford: The Clarendon Press, 1926. xxi + 357 pp., with numerous plates and maps. 21s. net. **99**

These regions are of peculiar interest to the historian of culture, since in them is to be sought the key to the relations between the populations of the interior of our continent and those of the Ægean. The classical references to Thracians and other tribes which Mr. Casson has assiduously assembled are, however, merely tantalising fragments. Apparently nothing is known of their pottery, which we should particularly like to see. On the other hand, "prehistoric" sherds abound in the locality and the author has materially augmented their number by his own valuable excavations in the Vardar valley. The results of these are here republished, together with some account of the Bulgarian excavations along the Maritza and of the antiquities of Bosnia. The remoter region of the Middle Danubian plain is left as a *terra incognita* in which it is, as usual, assumed that a precocious bronze culture flourished from an early date. While in this respect Mr. Casson follows the orthodox lines of archæological speculation, he deviates from current systems in describing a bow fibula with two twists as a "bow fibula of the simplest form" (p. 147), and assigning a copper axe of a type found with copper flat celts to the "full Bronze Age" (p. 297). The historical inferences based on the exiguous archæological material cannot be expected to be very convincing, but suggest interesting lines for further research. V. G. C.



FIG. 1.—CINERARY URNS FROM VATTINA. (3/20)

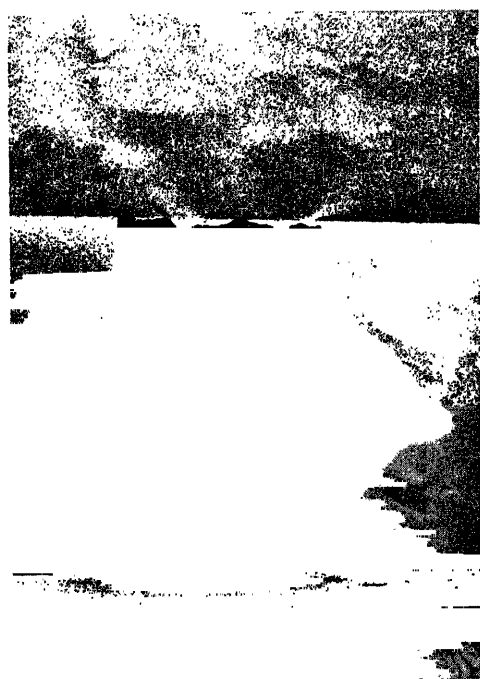


FIG. 2.—CINERARY URN FROM VRŠAC.

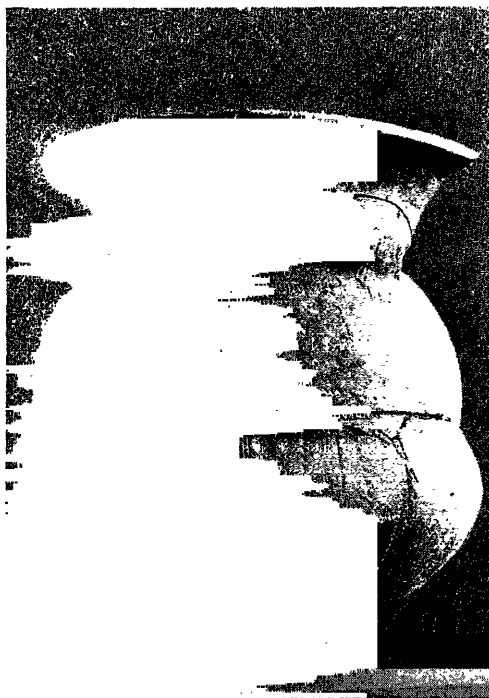


FIG. 3.—CINERARY URN FROM TEMES KUBIN. (1/3)



FIG. 4.—POTTERY FROM VATTINA

TRACES OF THE ARYANS ON THE MIDDLE DANUBE

ORIGINAL ARTICLES.

With Plate K.

Europe, Eastern: Archæology.

Childe.

Traces of the Aryans on the Middle Danube. By V. Gordon Childe, 100
B.Litt. With Plate K.

The impression of a cord constitutes such a simple and obvious means of decorating pottery that no ethnological conclusions can be drawn from its use alone. But in northern and eastern Europe, between the Rhine and the Volga, this device is employed for the ornamentation of vases of specialised form—beakers and amphoræ—which are frequently associated with special grave types, and a definite set of stone implements and peculiar ornaments. The term “corded ware” is technically used to designate only the pottery of this well-defined cultural group. Normally the pottery in question is associated with “neolithic” artefacts such as stone battle-axes and celts of flint or hard rock, but copper ornaments are far from rare in graves with corded ware.

Corded ware seems to have been fashioned by a nomadic or semi-nomadic folk who lived primarily upon the great North European plain. South of the ranges that encircle the Middle Danube basin true corded beakers have been found only on the plain of the Upper Tisza in County Szabolcs, Hungary (in Nyiregyháza Museum); Dr. Roska has identified sherds with cord impressions at various sites in the valleys of the Alt and Maros in Transylvania, and a sort of corded beaker occurs in one of the older lake-dwellings on Laibach Moor. The last named appears in a neolithic context (though whet-stones were found at the same site); the Transylvanian sherds belong to the “chalcolithic” culture that succeeds the painted pottery of the Erösd style (*P.Z.*, xvi., p. 86).

I am to-day able to add to this series of Middle Danubian finds a set of characteristic amphoræ from the urn-fields of Vattina and Vršac (Versec) in the Banat and that of Surčin in eastern Syrmia. These vases, which apparently served as cinerary urns, correspond so closely in form and decoration to the corded amphora as we meet it in Thuringia, Bohemia and Poland, that a genetic connection seems certain. At the same time they seem to belong to a distinctly later epoch than their more northern relatives. Unfortunately the excavation of the Middle Danubian urn-fields was seldom scientific, so that closed tomb-groups are rare, but the implements and ornaments from all the cemeteries in question illustrate the types that constitute the Middle Bronze Age that began not much before 1500 B.C. Moreover, one of the cord-ornamented urns from Vattina contained a pin with swollen and perforated neck, a bracelet with thickened ends (*Stollenarmband*), a heart-shaped pendant and a small razor, all injured by the funeral fire (Fig. 1). Of these the pin, bracelet

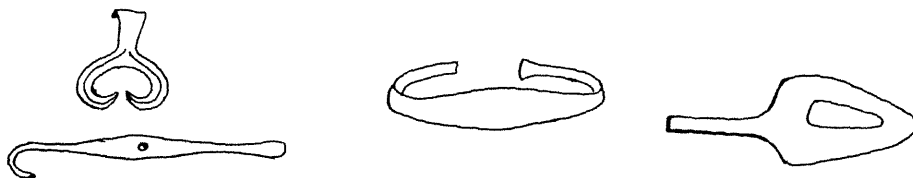


FIG. 1.—BRONZES FOUND WITH A CORD-ORNAMENTED URN AT VATTINA.

and pendant are characteristic of the earlier phase of the Middle Bronze Age called by Reinecke Period B. The razor is not unlike the *terramara* form but is more primitive and might therefore be somewhat earlier. Hence, even admitting that the bulk of the northern corded ware belongs to the latest phase of the neolithic period, as is generally agreed by Bohemian, German and Polish archæologists, it

is clear that a considerable interval of time separates our amphoræ from the more northerly specimens.

That gap explains the divergencies between the Danubian amphoræ and the Thuringian or Polish. The carefully moulded rim of the former cannot be matched in older specimens, and some of these late urns exhibit a curvilinear decoration of vertical plumes which is quite unique. In a few cases too the true cord impression has been replaced by imitations executed apparently with a coiled wire. Nevertheless, our urns preserve the arrangement of the handles round the middle of the belly characteristic of the standard corded amphora and the horizontal impressions round its neck. Moreover the skeuomorphic pattern seen on the specimen here illustrated can be paralleled in Thuringia itself (*cf.*, e.g., *J.S.T.*, i, pl. XII, 12) where secondary handles at the base of the neck are likewise encountered. Finally, while corded ware is normally found in graves containing contracted skeletons, it accompanies cremated remains, as in the Middle Danubian urn-fields, in some graves in Thuringia, the Rhine basin and Poland (*Z.f.E.*, 1906, pp. 321ff., Götze, Höfer and Zschiesche, *Die vor- und frühgeschichtliche Altertümer Thüringens*, p. 132, *Przeglad Arch.*, ii, p. 296).

The discovery of descendants of corded ware on the Middle Danube is of peculiar interest. I have been led to the conclusion that, in so far as the people, whose existence is postulated on philological grounds and who are variously termed Aryans, Indo-Europeans or Wiros, can be identified with any of the cultural groups known in Europe, the makers of corded ware best fulfill the conditions deduced for the culture of our linguistic ancestors (*The Aryans*, 1926). Moreover, I could prove with the aid of German authorities the affiliation between the makers of corded ware and the authors of the Southwest German "tumulus culture" of the Middle Bronze Age as well as with the contemporary barrow builders of the North. A similar linkage with the Middle Bronze Age people of the Hungarian plain was particularly desirable in view of the important place assigned by Peake to that area in the "wanderings of the Wiros." The urns from Surčin and the Banat which I am allowed to publish here through the courtesy of Dr. Milleker, Keeper of the Museum at Vršac and of Prof. Vulic who has kindly allowed me to use photographs prepared for him, supply the missing link. Plate K, figs. 1 & 2.*

This demonstration acquires additional importance in view of the patent affiliation of the Middle Danubian urn-field culture to that of the *terremare* (Plate K, fig. 4) and later to the Villanovan (Plate K, fig. 3), both of which are indubitably to be attributed to Italici.

V. GORDON CHILDE.

Technology.

Harrison.

Variations and Mutations in Invention. By H. S. Harrison, D.Sc.

101

In some recent notes in MAN (1926, 74), I suggested that the development of tools and other appliances has been due to changes of two kinds, to which the terms "variations" and "mutations," respectively, may be applied. The suggestion could not then be elaborated, but it seems worth while to attempt a justification, if only with the hope of arriving at a clearer understanding of the implications of the expression "independent invention." In making use of biological analogies, for convenience, I am fully aware that, like other analogies, they must not be looked too closely in the mouth.

In ordinary diction, and in dictionaries, the word "invention" is given such an ill-defined range of meaning that it is past praying for as a term of precision. Here

* Since this note was in the press a further link has come to light in the shape of a jug of proto-Pannonian type ornamented with cord impressions, published in *M.A.G.W.*, 1926, p. 221.

I shall use it in its abstract sense; and in its particular sense only with reference to complete tools or appliances such as ploughs and pianos, kaiaks and kymographs. For single "inventive" steps such as have played a part in the development of many artefacts, I shall use the term mutation.

In the notes referred to above, I expressed the view that the modern inventor has the advantage over his early forerunners in that he forms some conception of what he aims at, and his progress tends, or pretends, to be wholly "directional." Early man, less sophisticated and less dissatisfied, was dependent on promptings from without, and looked ahead shortsightedly, if at all. Obtrusive discoveries and variations must often have been slighted or ignored, since their importance was not grasped. We may even surmise that an invention or a method, once established, was unconsciously regarded as possessing a stability comparable with that of natural objects or phenomena, and that mutational improvements were revelations that visited only the adventurous. Nor can we assume that necessity was the mother of invention, since man only becomes aware of a need when he has happened upon a means of satisfying it.

Restricting ourselves for the moment to tools and appliances, it is obvious that slight advantageous changes must often have been made by chance, insofar as the maker had no intention of producing them, or was influenced by some consideration that had no relation to the use or efficiency of the artefact; variations of this kind would be especially likely to occur in respect of properties of dimension, proportion, and number. If such a variation was found to serve a useful (or even an ornamental) purpose, it would perhaps survive by selection, and it may have served as a starting point for similar "purposive variations," or "adaptations," tending in the same direction. That this conscious process of selection was rare in early times may be deduced from the extreme slowness of inventive progress. As regards variations, of whatever kind, they may be pictured as originating and accumulating without any exercise of the inventive faculty, though they eventually supplied the material for this faculty to work upon; there are, however, tools and appliances which in their completed form, and ignoring their conjectural method of origin, seem to deserve the title of inventions, though they may well have evolved entirely by variational changes. Amongst these, the pick, the push-quern, and the pestle and mortar, may be cited. Similarly, early methods and processes depended almost entirely on discovery and variation. I would here emphasise, with apologies, the obvious fact that at the root of all man's tool-making and productivity lie observation and discovery, and that without applied discoveries no progress could have been made. But the early discoveries (as well as many that were not so early) although they involved finding out, were not the results of research, though curiosity, no doubt, played a part.

Mutations may be distinguished from variations on the one hand by the fact that, taken singly, they are considerably more important in their influence, and on the other, that they present an effect of abruptness, and even discontinuity—real in relation to the evolution of a particular appliance, but, as we shall see, not so real if viewed from a wider standpoint. We may, therefore, regard mutations as conspicuous steps in advance, of such a nature that their origin by a summation of variations, within the phylogeny of an appliance under consideration, does not seem probable. This is sufficiently explicit for my purpose, though I do not pretend that the definition is rigidly scientific.

Let us take an example:—At a critical stage in the evolution of the rotary quern, two essential improvements were made, associated in their structural relationships, and with a functional overlap. A hole was made through the centre of the upper stone, through which grain could be fed to the mill; a peg was fixed vertically in the centre of the lower stone, and since this passed upwards into the

hole in the upper stone, it served as an axis of rotation. In relation to the evolution of the quern, it is difficult to imagine that either of these changes arose by the accumulation of variations, and each of them is sufficiently important to be called a mutation. It is possible, of course, that there was only one mutation, viz., the provision of the axial spindle and a hole for its reception, the use of the latter as a channel for the grain being secondary; but this is not a point of importance from our present standpoint. Nor can we pause to speculate upon the exact genetic relationship of this mutation to other rotary appliances.

Other examples might be given, but this is sufficient to suggest the conception of a mutation as a fertilising idea or device introduced into an appliance from without. If, as would usually be the case in early times, the mutation owed its origin to a process of variation in some other appliance, we may say that a mutation is a variation-product transferred and adapted to new conditions; in advanced inventions, however, mutation products or complexes are also liable to transfer, as we may see on all sides. Many ethnologists who have endeavoured to trace the origin of particular inventions or processes to observation and discovery have shown their belief in the development of artefacts by applied discovery and variation, and these were certainly the main factors in the origin and evolution of simple appliances. Although at the moment I am laying stress upon mutations, I regard them as only beginning to come into their own when appliances of at least some slight complexity made their appearance. It is in the transfer and adaptation of mutations—originally a process of hybridisation—that the inventive faculty is displayed, though in modern days the nature of the process is obscured by the extent to which mutation concepts have become “common knowledge.”

A few words may be said as to certain commonly-occurring ideas or devices which arose from discovery or variation, and which were in the first instance transferable as mutations. The simplest of these are the edge and the point, and they became assimilated—common knowledge—at such an early period that the habit of producing them when necessary became almost an instinct. Less simple, and later in origin, were tangs, shouldered hafts, lashings, vertical sockets, and other variation-products. As mutations these are not impressive, but there must have been cases in which such features were utilised in the improvement of appliances that had developed to a certain point without them. Even such a simple convenience as a handle may have been first developed in basketry, and transferred to pottery as a mutation; the loop of the bronze palstave falls into the same class. The process of transfer rises, however, in some of these cases very little above the level of imitation, and the inventive faculty was only marginally involved. At the same time, the importance of such transfers must not be underestimated; in a region where, for example, tangs and sockets, and other hafting devices, might all be developing together, their inter-relationships would be too entangled for analysis, but any one of them, at any definite and useful stage, might be carried to a region where a career as a mutation was open to it, and where it would also be liable to variation under new influences. An introduced type of socketed copper implement might bring about changes in methods of hafting, always supposing that the new region was ripe for such changes—that metal tools were already in use, and that the application of the socket idea did not involve great modifications in the form of the tools to which it might be applied.

If discovery and variation were the chief factors in material progress we may conclude that in prehistoric and early historic times man's inventive faculty had very little scope. Only slowly did there evolve a few ideas and devices that could pass as mutations from one appliance to another, and the earliest of these were of great simplicity. The conception of early man as being driven by the common tendencies of the human mind to invent the same tools, or produce the same artefacts,

in similar environments, seems to be based on the one hand on an overestimate of the opportunities and the enterprise of the inventor, and on the other, on a misleading deduction from the directional character of modern inventive methods. The operative common tendencies were chiefly observation and imitation, neither of which is progressive, to which we may add a flavouring of inquisitiveness. There was always a conservative drag on the inventive coach, and it was, moreover, only by a combination of fortunate circumstances that advances could be made. Variation was a sluggish process, with no certain outcome, and a mutation—itself a variation-product—could only be adapted to an appliance that was ready for it.

Since early inventions were the end-results of certain lines of progress, chiefly variational, to speak of “independent invention,” as though it was merely a matter of the reproduction of single creative acts, is to begot the issue even more than is done by the use of the term itself. Every appliance and product has reached, or passed through, one or more phases of individuality for which the term “expression point” is convenient. Such a phase marks the attainment of a well-defined form or constitution, and it may be prolonged in use indefinitely, undergoing improvement perhaps, but remaining always the same thing; it may also give rise to a new type of appliance, a new invention, by variation and mutation, though the two processes need not of necessity lead to new types. As an example, the pick remains a pick, whether it is made of one piece of wood or consists of a wooden haft with a socketed iron head. But the simple pick gave rise to the plough, another invention or expression point. The latter term is convenient as having a wider range of application than the former, which is scarcely appropriate except for tools and other appliances. An expression point was reached in the development of the art of writing when ideograms were established in use, and their introduction into an alien system of pictography might initiate a variational process leading to a hieroglyphic system, which would present little or no evidence of relationship to other systems, though it would owe its origin to diffusion. Other expression points are the use of fire to harden clay vessels, and the sowing of seeds with the intention of raising a crop; ceramics and agriculture, respectively, radiate from these two points, and here was the opportunity for diffusion.

An expression point was a point, or stage, on a line of variation, though in some cases it owed its individuality and its relative stability to a mutation. The lines of variation from any expression point were often numerous, but few, or perhaps only one of them, could lead to a new expression point; others led nowhere in particular, though they may have resulted in an increase of efficiency or of artistic merit. If early man had known what he was trying to invent, he would have laid down the lines for variation, and its course might have been the same in China as in Peru. But he didn't know, and whilst, for example, one people arrived at the saddle-quern and could get no further, another got on a line that led to the rotary quern, and ultimately to the windmill; one “invented” tied cloth, but the true loom was not on that line; one specialised in chipping flint, and had to be taught how to shape stone by friction. There were, therefore, not only lines of variation that were unfruitful, but others which led to artefacts which had a past but no future; and these, as in the case of the saddle-quern, might be so efficient for their purpose, and so specialised in form, that metamorphosis by variation or mutation could scarcely occur, with the result that they served as an obstacle to the development of competing appliances having greater potentialities—in this case the rotary quern. Even though we may allow early man some little foresight, it could not save him from dead-ends of this class.

I make no suggestion that the human mind does not react in the same kind of way to similar environmental stresses; but when, except in adjacent areas, are two environments, cultural as well as natural, the same, as distinct from generally similar?

When, in fact, except in modern civilisation, are conditions so nearly identical that the common originaive tendencies of the human mind, such as they are, can obtain their opportunity to run on parallel lines? Alluvial plains, deserts, forests, all have their characteristic features, but in different parts of the world there is diversity in animals, plants, minerals, and in their products. Such differences influence details in material contrivances, and the momentum of invention was insufficient to overcome even trifling causes of deflection. To disbelieve in independent development as having been a normally recurrent phenomenon does not involve a dogmatic denial of its possibility in sporadic cases. Coincidences occur, though strings of them are not common. Some appliances are so simple, however, that parallelism in origin and development might have happened—if diffusion was not first in the field—in spite of the infrequency of the conjunction of appropriate conditions, and in spite of the fortuitous nature of the variation process. But the independent development, through series of variations and mutations, of such inventions as the spindle, the loom, the bark canoe, the flageolet, and others, calls for a long arm of coincidence with a universal joint and a telescopic extension; either that, or a belief, in the face of the archæological evidence, that the inventive faculty of man is a persistent and farseeing force, and not a casual and myopic opportunism. As for the independent evolution in one region, however transatlantic, of a number of such appliances—to say nothing of agriculture, architecture, and pottery-making—the attempt to believe it seems quixotic.

In conclusion, I would suggest that this provisional analysis of the nature of inventions and their methods of growth, provides some arguments against the theory of independent invention. Those who regard the evolution of man's artefacts as being due to the consistently directive exercise of the inventive faculty, who place human ingenuity and foresight in the forefront, will no doubt disagree, and will regard man as being master of his inventions. If it is so, the slowness of inventive progress, down to quite recent times, must give us cause to wonder; even, perhaps, to wonder why Julius Cæsar did not forestall Louis Blériot in the manner of his landing on our shores.

H. S. HARRISON.

Europe, Eastern: Archæology.

Franz.

Remarks on a South-East European Ceramic Type. By Leonhard Franz, Ph.D., Vienna.

102

In Thessaly we meet a vase which distinctly recalls a leather pouch. Figure 1 shows such a vessel; it comes from Lianokladhi and belongs to the class of "red on white" ware.* The shape was evidently chosen to facilitate pouring. The same type appears at Hagia Marina; the specimen reproduced in Figure 2 is provided with a handle, a deviation from the Lianokladhi variety which is of quite secondary importance in view of the general agreement in shape. Comparable vases, peculiarly reminiscent of a skin or leather pouch, are known from Dimini too (Fig. 3). We encounter the same type in a rather more developed form in the late Neolithic culture of Hungary; Fig. 4 shows an example.† Here the rim, which at Hagia Marina and Lianokladhi is still low and of an even height all round, has become higher. The upward curve of the rim from the base of the handle gives the impression of a sloping lip drawn out into a sort of spout. At the same time the handle has been carried higher up and has so become more convenient for holding.

* Lianokladhi I contains also Hagia Marina (Early Helladic) ware like II; the third stratum yielded Minyan ware which provides a basis for chronology: Lianokladhi III would correspond roughly to M.M.II while strata I and II would fall in M.M.I and the preceding epoch. *c.f.* Wace and Thompson, *Prehistoric Thessaly*, pp. 17ff.

† H. Schmidt, *Schliemanns Sammlung Trojanischer Altertümer*, No. 3585.

In contrast to their Thessalian prototypes, the Hungarian vases have lost all painted ornamentation. Similar forms still appear in the *Buckelkeramik* of Troy VII. But vases like Fig. 5 from Phylakopi in Melos also belong to the same series. That might at first sight appear a rash assertion, but it is supported by the existence of

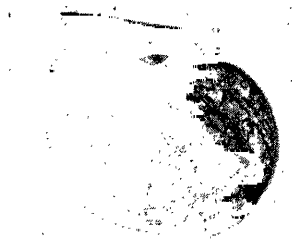


FIG. 1.—AFTER WACE-THOMPSON, *Prehistoric Thessaly*, FIG. 119.



FIG. 2.—AFTER D. FIMMEN, *Die kretisch-mykenische Kultur*, FIG. 128.



FIG. 3.—AFTER TSOUNTAS, *Dimini and Sesklo*, FIG. 206.

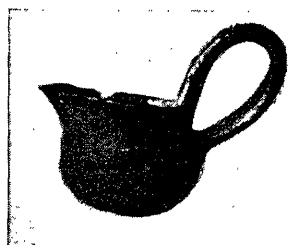


FIG. 4.—AFTER H. SCHMIDT, "Tordos," *Zeitschrift für Ethnologie*, 35, 1903, P. 456, FIG. 36, 37.



FIG. 5.—AFTER O. MONTELIUS, *La Grèce préclassique*, I., PL. 84, 11.



FIG. 6.—AFTER O. MONTELIUS, *La Grèce préclassique*, I., PL. 84, 9.

intermediate forms such as Fig. 6. In the latter the body still preserves its characteristic shape. Only the neck has been contracted and the handle reduced in size. Limiting forms like Fig. 5 arise through the vertical depression and exaggerated horizontal extension of the belly. The position of the handle at the base of the neck and the conformation of the rim agree in both cases.

LEONHARD FRANZ.

Religion.

Father, Mother, and Child. By A. R. Radcliffe-Brown.

Brown.

In MAN for 1925 (80) Mr. Lyons points out that the ritual of Murua suggests that the natives of that island regard "the incidence of childbirth as " being something in the nature of a welding of personalities—in other words, a " trinity of father, mother and child." Is it perhaps worth while to point out that the same conception is found in a great many primitive societies, probably in all, and that an appreciation of it is essential to the proper understanding of the meaning and function of many primitive customs?

The Murua ritual shows that there, as in many other societies—as, for example, in the Andaman Islands—the family does not really exist as a properly constituted group until the birth of the first child. The relationship between husband and wife is one that is not simply created by marriage but does not exist in its complete form until a child has been born, and is an indirect relationship through the child or children. This conception is illustrated by the custom of teknonymy, by which one of the spouses (and frequently his or her relatives also) addresses

the other spouse only as "mother of" or "father of" the child. A great number of other customs could be mentioned which show the same thing, namely, that the relationship between the husband and wife is through the child.

The imposition of a special taboo in which two or more persons share is one of the commonest methods in ritual of establishing a special solidarity between them. Thus, a taboo that is obligatory on all members of a clan, whether it is a totemic taboo or one of some other kind, is one of the means by which the solidarity of the clan is established and maintained. The establishment of any solidarity between two or more persons involves the partial assimilation of their social personalities. Inversely, the imposition on an individual of a special taboo which he does not share with anyone else is a means of differentiating his social personality from all others, making him an individual.—as, for example, in the so-called individual or personal totemism of Eastern Australia, where, however, only medicine men and women have such individualising taboos. Thus, the custom described by Mr. Lyons from Murua is only an example of the one general principle that underlies totemic and similar taboos all over the world. It is all the more interesting on that account, because it confirms the view that some of us hold that, however such customs may vary in detail in different cultures, the same fundamental sociological laws or principles underlie them all. The imposition of a special taboo on the father, the mother and the child is, as Mr. Lyon points out, a means of establishing a special solidarity between them and thereby producing a partial assimilation of their social personalities. It is by this ceremony that the family is definitely constituted as a complete social group made up of a trinity of persons.

It is only the first child that counts. First, because as soon as one child is born, the trinity is complete. Secondly, because the children of one family are conceived in primitive societies as being, as it were, multiples of a single personality, differentiated only by sex and order of birth.

It has not been sufficiently noted, I think, that this conception of the family as a trinity exists even amongst polygynous people. There are two ways in which the institution of polygyny is reconciled with this conception of the family. (There may possibly be others that I have not noted.) One is to regard the wives of the same husband as being merely multiples of one personality. This is, of course, much easier if they are sisters, and we here see the function of the sororate in the Australian form where a man marries two or more sisters—where, in fact, marriage with the eldest of a number of sisters gives him the right to marry all the others. By the fact of their being sisters, children belonging to one family, they are multiples of a single personality.

The other method is to separate the polygynous household into a number of units (houses), each with a wife and her children, the units being held together by having a husband in common. This is the form typically taken by the polygynous family in Africa. Normally, each "house" is a separate entity, having its own social and economic life separate from that of other "houses" of the same household group. But in Africa it would seem that the other method mentioned above may be adopted in certain circumstances (a) if a man marries two sisters, in which case the two wives are, as it were, multiples of one personality, so that there are not two separate houses but really one, and (b) where a wife is childless and another wife is obtained to bear children that can be counted as children of the first.

Finally, a word may be added on polyandry. The most usual form is the adelphic, in which the husbands of one wife are brothers. The position here is that two or more brothers, by the fact that they are brothers, born in one family, are multiples of one personality, and are therefore merged with one another in

exactly the same way as two sisters who are married to one man in Australia, or even, in some instances, in Africa. The rare cases of anadelphic polyandry present difficulties that cannot be discussed in a brief note, but I have not found in them, or for that matter elsewhere in primitive societies, any real exception to the general law that the family is everywhere conceived as being essentially a trinity of father, mother and child.

A. R. RADCLIFFE-BROWN.

Anthropology, Physical: Disease.

Cunningham.

Some Factors in Racial Immunity and Susceptibility to Disease.

104

By Lt.-Col. J. Cunningham, B.A., M.D., I.M.S., Director, King Institute of Preventive Medicine, Madras.

Although the study of medicine in itself is not of much interest to the anthropologist, the influence of disease upon racial distribution, and even the degeneration and ultimate extinction of races, is universally recognised, and forms a connecting link between the two sciences.

Ripley has considered this side of the subject in his study of the future of the European races and the possibility of their being able to extend to the tropics and maintain themselves there by adaptation and acclimatisation. He decided against such a possibility. Much of the evidence upon which he based his opinion is now out of date, and it occurred to me that a review of our present knowledge of the factors which govern the relative immunity and susceptibility to disease of different races would be of some interest, more especially as the whole question of the colonisation of the tropics by the white man is once more in evidence, and is being keenly debated in the light of recent advances in our knowledge of tropical medicine.

It is natural that the comparative liability of the European and native to indigenous diseases should take a prominent part in a discussion of this kind. Such a comparison, however, although certainly relevant to the ultimate point at issue, does not afford very satisfactory proof of a natural or innate immunity. In many cases, where two or more races are intermingled, one is obviously living at a disadvantage. In such cases an adverse environment may introduce secondary factors, such as a loss of resistance to infection, which of themselves may cause an increased disease incidence. A true racial immunity can only be gauged where, other factors being equal, each race is living under conditions to which it has already become adapted. A satisfactory investigation thus becomes a matter of difficulty, but is not impossible for Shrubsall's inquiry into the relative susceptibilities of the blondes and brunettes affords a classical example of the type of investigation required.

Tropical countries in general, and India in particular, are likewise very suitable for investigations of this type. The contrast in races and their environment is much greater than can be found in European countries. The greatest differences in habits, diet and sanitary standards may be present in communities living side by

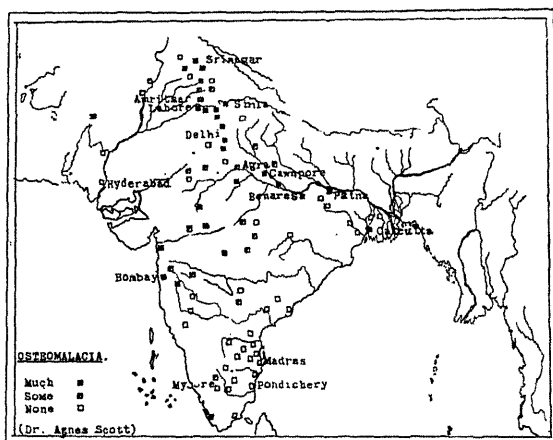


FIG. 1.—SHOWING THE DISTRIBUTION OF OSTEOMALACIA IN INDIA. AFTER DR. AGNES SCOTT. (*Indian Journal of Medical Research*).

side, and, conversely, the same or closely related races may reside in parts of the country showing vast differences in climate. These factors permit of controlled observations which would be impossible elsewhere.

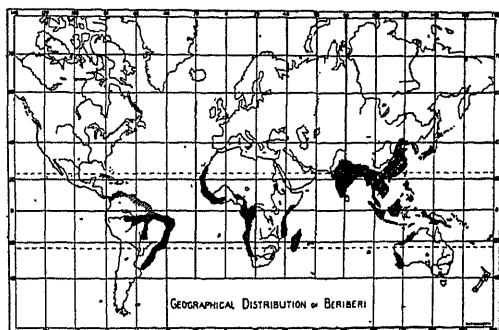


FIG. 2.—SHOWING THE GEOGRAPHICAL DISTRIBUTION OF BERI-BERI. AFTER VEDDER. ("PRACTICE OF MEDICINE IN THE TROPICS," BY BYAM AND ARCHIBALD).

child-bearing period and which consists in a softening of the bones, leading to great deformity, is comparatively frequent in certain parts of the East. The exact cause of the affection is unknown, but the morbid process has to do with a defect in the calcium metabolism. Scott, who has investigated the disease in India, has shown that it occurs chiefly in those parts of the country where the rules of "purdah-nishin" are more or less closely observed. Thus, the wives of well-to-do banias and khatri (kshatriya), both belonging to the Hindu community,* are the chief sufferers in the Punjab, and the Borah Mahommedans in western India and the Central Provinces, whereas the Bengalis and Dravidians, whose women are allowed much more freedom, are hardly affected at all. (See Fig. 1.) Whatever the primary cause may be, it is the "women who lead sedentary secluded lives in unhygienic surroundings" who "appear to be chiefly affected by the disease." According to Wampler and Maxwell, osteomalacia is also very prevalent in certain

Racial peculiarities may, however, have a modifying effect upon the incidence of disease. Certain diseases—for example, one or two of the deficiency diseases—may be mainly due to such causes. It is necessary, therefore, to consider the influence of all such secondary factors before the evidence of a true racial immunity can be accepted. Numerous examples of the baneful effect of racial habits can be cited. Three, however, will suffice to illustrate this point.

OSTEOMALACIA, a peculiar disease which chiefly affects women during the

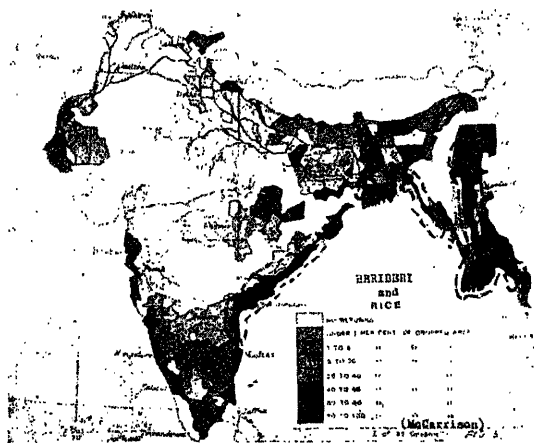


FIG. 3.—THE RELATIONSHIP OF ENDEMIC BERI-BERI TO THE RICE-GROWING AREAS OF INDIA. AFTER MCCARRISON. (*Indian Journal of Medical Research*).

Note.—The black areas surrounded with a dotted line represent the endemic areas. These are: an area round the mouth of the Irrawaddy river extending northwards, an area along the upper part of the Burmese coast, a small area in the region of Calcutta, and one extending along the upper part of the east coast of the Madras Presidency.

* Dr. Scott found 27 out of the 83 cases she examined "were strictly purdah. Of the remaining 56 the majority were Hindus of the higher castes. The latter do not keep the strict purdah observed by the Mohammedans in upper India, but they leave their houses very seldom: a marriage or a death in the family, or a religious festival, being almost the only occasions on which the women go out of doors."

parts of China. The same predisposing causes appear to exist there also, but in this case the seclusion is not, as far as I am aware, enforced by racial custom.

As an example of a disease dependent on racial peculiarities in diet we may take **BERI-BERI**. Beri-beri is caused by the continued use of foods, such as polished rice, which are deficient in the anti-neuritic vitamin.

The disease is thus commonly found in the East Indies, China, Japan and the Malay Peninsula (see Fig. 2), where rice is used as the staple article of diet. The vitamin is contained in the cortex of the grain, which is removed to a greater or less extent according to the different processes of milling adopted by different communities.* Outbreaks of beri-beri follow the too exclusive use of polished rice and can be cured by the substitution of undermilled rice. McCarrison, however, has recently come to the conclusion that some additional factor, at present unknown, is also involved in the process, at least as far as the disease in India

is concerned. In support of his opinion he shows that the endemic areas of the disease are limited to certain parts of Madras and Burmah, which are very small when compared with the rice-growing and rice-eating populations (see Fig. 3).

He fully acknowledges, however, the paramount influence of a diet deficient in vitamin B, quite apart from the conception of the additional causative factor.

A communal distribution of disease may also be determined by **RELIGIOUS** or **CASTE** factors. Social ostracism may cause the isolation of members of the same community to an extent which can hardly be realised in this country. A curious example of this kind recently came to my notice during an investigation of a new endemic focus of kala azar in southern India. The disease had been present in the town

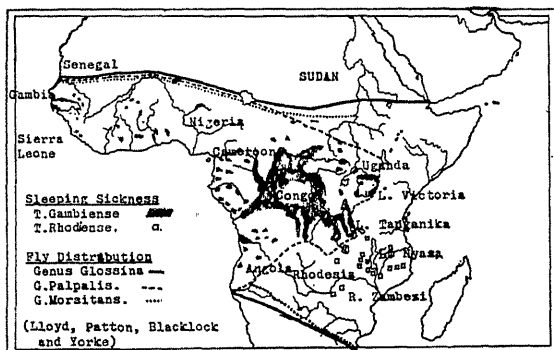


FIG. 5.—THE RELATION BETWEEN THE DISTRIBUTION OF SLEEPING SICKNESS AND THE TSE-TSE FLY AREAS.

AFTER LLOYD, PATTON, BLACKLOCK AND YORKE.

("STUDY OF MEDICINE IN THE TROPICS," BYAM AND ARCHIBALD).

since 1882, but the Mahommedan community alone were affected, although both Hindus and Mahommedans lived in the closest proximity to each other (see Fig. 4). The habits of the latter as traders had brought them in contact with other endemic foci, such as Madras and Calcutta, while the Hindus, whose occupations as

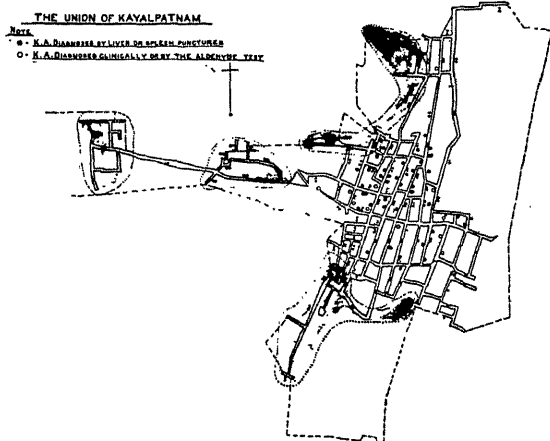


FIG. 4.—SHOWING THE COMMUNAL DISTRIBUTION OF KALA AZAR IN THE VILLAGE OF KAYALPATNAM, SOUTHERN INDIA.

The shaded areas represent areas occupied mostly by non-Mahommedans.

* Raw polished, or decorticated, rice is most deficient in the vitaminic principle, while undermilled rice contains a plentiful supply. Parboiled rice, where the grain is subjected to a preliminary steaming before polishing, falls between the two.

agriculturists had kept them in the locality, had never been infected, presumably owing to the social barrier erected by religious differences existing between the two communities.

The effect of *HYGIENIC SURROUNDINGS* in modifying the incidence of infective disease is everywhere acknowledged. Plague, cholera and many other diseases have disappeared from Europe as a consequence of the higher sanitary code adopted by the European races. The persistence of these diseases amongst the tropical races is amply accounted for by the complete indifference displayed by the native to the elements of sanitary science.

CLIMATE may operate in a variety of different ways. In the first place, the prevalence of insect-borne diseases must be determined by the bionomics of the insect host, and meteorological conditions are amongst the more important factors which govern the habits, and therefore the distribution, of the insect world. The close relationship of the distribution of the insect and the disease for which it is responsible, is well illustrated in the case of African sleeping sickness, which is prevalent only in those parts of the country infested by the tse-tse fly (see Fig. 5).

The climatic factor is of even greater importance where the pathogenic organism undergoes some life-cycle in the insect. In many cases development takes place under certain conditions of temperature and humidity only. In the absence of these, retardation or cessation of growth occurs. The diminution of malaria during the colder months is an example of this fact.

Even in diseases caused by bacteria, meteorological conditions play a considerable part through the agency of chills and other depressing influences. The seasonal prevalence seen in many diseases is undoubtedly due to factors of this kind.

Evidence in favour of a true *racial immunity* can best be sought amongst the infective diseases; for, in addition to the fact that the majority of diseases are of this nature, parasitism, or the invasion of one species by the individuals of another, is almost always opposed in nature, and evidences of this opposition are frequently to be found.

The study of these processes constitutes the science of immunity, and, as some knowledge of this subject is necessary for a proper appreciation of the evidence I wish to bring forward, I trust I shall be excused if I digress for a moment to refer to one or two salient facts dealing with natural and acquired immunity as it occurs throughout the animal kingdom.

(To be continued.)

Archæology: Bronze.

Armstrong.

Analyses of Bronze Implements and Foundry Metal. By A. Leslie
Armstrong, F.S.I., F.S.A.

105

In connection with the work of the British Association Committee on the Distribution of Bronze Age Implements, I am able to record the composition of six bronze implements and three fragments of founders' metal. These valuable records have been made possible by the generous assistance of Professor Cecil H. Desch, F.R.S., Professor of Metallurgy at the Sheffield University, who from time to time has kindly analysed specimens sent to him, and by the co-operation of Mr. T. Sheppard, M.Sc., Keeper of the Hull Museums, the late Mr. H. Ling Roth, Keeper of Bankfield Museum, Halifax, and Dr. W. E. Collinge, F.L.S., Keeper of the Yorkshire Museum, York, who have each willingly consented to the examination of specimens in their charge.

(1) A palstave, part of a small hoard found in 1864 in the Great Park, Windsor, until recently preserved in Hull Museum, but now transferred to the Ashmolean

Museum, Oxford.* The implement is patinated a dull green, the metal having a brittle and altogether abnormal appearance, which the analysis confirms. Professor Desch states that as the composition was a rather puzzling one, he had the analysis repeated very carefully, "but the total still fell far short of 100, which is apparently "due to oxide. The casting really represents a failure, as the amount of oxygen "and also of sulphur is so great that the metal is hopelessly brittle." His final results are:—

<i>Element.</i>						<i>Per cent.</i>
Copper	-	-	-	-	-	78.79
Tin	-	-	-	-	-	16.49
Nickel	-	-	-	-	-	0.49
Lead	-	-	-	-	-	0.09
Iron	-	-	-	-	-	trace
Zinc	-	-	-	-	-	nil
Sulphur	-	-	-	-	-	0.68
Total						96.54

The remainder consists apparently of oxide, for which there is no accurate method of analysis.

(2) A palstave, from the Bankfield Museum, Halifax, one of a small hoard found at Westercroft, Shelf, near Halifax (Museum No. A.M. 2277). Appearance not quite normal, patinated light green, lustrous only in places, edges splintered and metal obviously brittle. These features are common to the three specimens from this hoard, and are probably due to the excess of sulphur revealed by the analysis:—

<i>Element.</i>						<i>Per cent.</i>
Copper	-	-	-	-	-	86.32
Tin	-	-	-	-	-	12.14
Sulphur	-	-	-	-	-	1.50
Lead	-	-	-	-	-	trace
Iron	-	-	-	-	-	nil
Nickel	-	-	-	-	-	nil
Zinc	-	-	-	-	-	nil
Total						99.96

(3) A palstave, from the Hull Museum, believed to be Irish, purchased in Darlington (Museum No. 107). Of normal appearance, lustrous patina, brown to green in colour:—

<i>Element.</i>						<i>Per cent.</i>
Copper	-	-	-	-	-	80.25
Tin	-	-	-	-	-	16.39
Lead	-	-	-	-	-	0.16
Iron	-	-	-	-	-	nil
Nickel	-	-	-	-	-	0.47
Sulphur	-	-	-	-	-	0.24
Zinc	-	-	-	-	-	nil
Total						97.51

* H. W. Franks, *Proc. Soc. Antiq.*, 1866. T. Sheppard, *The Naturalist*, 1922 and 1926.

(4) A socketed celt, from the Yorkshire Museum, York. Believed to have been found in East Anglia. Normal in appearance, smooth brown patina :—

<i>Element.</i>	<i>Per cent.</i>
Copper - - - - -	78.88
Tin - - - - -	9.68
Lead - - - - -	9.75
Iron - - - - -	1.04
Nickel - - - - -	nil
Silica - - - - -	0.24
Sulphur - - - - -	0.20
Total - - - - -	99.79

(5) A socketed celt, from the writer's collection. Found on the Yorkshire Wolds, near Driffeld. Patina dull green, surface much corroded :—

<i>Element.</i>	<i>Per cent.</i>
Copper - - - - -	78.77
Tin - - - - -	8.65
Lead - - - - -	6.15
Iron - - - - -	0.49
Nickel - - - - -	0.29
Silica - - - - -	0.40
Sulphur - - - - -	0.20
Total - - - - -	94.95

The Everthorpe Hoard.

Amongst the treasures of the Hull Museum is the complete hoard of thirteen socketed celts, a gouge and three lumps of founders' metal, found at Everthorpe, East Yorks, in 1842.* An examination of one of the lumps of metal (Museum No. 125), by Professor Desch, revealed the surprising fact that the metal was practically pure copper. Commenting upon the analysis, Professor Desch said : " It is remarkable in being almost pure copper, which is, I think, very unusual in English objects. Without looking up analyses, I cannot remember having seen one so rich in copper. Should it be true that pure copper was first made and then alloyed with tin, it would be very interesting, but this scarcely seems likely. It will be worth while to make an analysis of one of the implements from the hoard."

Upon acquainting Mr. Sheppard with this, he readily consented and forwarded a representative celt, and also the remaining two pieces of metal, for analysis.

Most of the celts in this hoard are unfinished foundry specimens, evidently " throw-outs," rejected on account of blow-holes and flaws in casting and reserved for re-melting. There is, therefore, reason to believe that they were part of the output of this particular craftsman and not objects he had obtained as scrap-metal, such as frequently compose the bulk of a hoard. It was one of these unfinished celts which was selected for examination and submitted to Professor Desch, together with the further specimens of foundry metal. His analyses are as follows :—

* T. Sheppard, *The Naturalist*, 1922 and 1926.

(6) Three lumps of metal from Everthorpe hoard (Museum Nos. 125, 124 and 98), weighing respectively 623·7, 510·3 and 1247·8 grammes :—

<i>Element.</i>				<i>No. 125.</i> <i>Per cent.</i>	<i>No. 124.</i> <i>Per cent.</i>	<i>No. 98.</i> <i>Per cent.</i>
Copper	-	-	-	99·19	98·98	99·16
Tin	-	-	-	0·055	0·07	0·05
Lead	-	-	-	0·035	0·00	0·03
Iron	-	-	-	nil	} Not estimated for Nos. 124 and 98.	
Nickel	-	-	-	nil		
Zinc	-	-	-	0·03		
Arsenic	-	-	-	trace		
Silica	-	-	-	trace		
Sulphur	-	-	-	0·68		
Total				99·99		

A full analysis of No. 125 was made, but as Nos. 124 and 98 obviously represented the same metal, viz., a very nearly pure copper, it was not thought necessary to estimate the other elements.

(7) A socketed celt from the Everthorpe hoard (Museum No. 121). Patina dull green to bronze :—

<i>Element.</i>						<i>Per cent.</i>
Copper	-	-	-	-	-	66·88
Tin	-	-	-	-	-	10·54
Lead	-	-	-	-	-	22·36
Iron	-	-	-	-	-	nil
Nickel	-	-	-	-	-	trace
Silica	-	-	-	-	-	trace
Sulphur	-	-	-	-	-	0·18
Total				-	-	99·96

When forwarding the analyses, Professor Desch stated :—“ The case of “ Everthorpe is peculiarly interesting, as there is no doubt that the irregular lumps “ from that hoard consist of pure copper, and the inference seems fairly clear “ that this copper was afterwards alloyed. The object 121 has evidently been “ made from a pure copper, but the amount of lead added is quite remarkable. “ I do not remember examining another specimen with so much lead.”

A. LESLIE ARMSTRONG.

REVIEWS.

Ethnology. Pittard. *Race and History.* By E. Pittard. 106 Translation by Miss Collum. His- tory of Civilisation Series. London: Kegan Paul, 1926. xxiii × 505 pp. 25 × 16 cms. 2ls.

This book attempts a task beyond any- one's power at the moment, and its value is therefore that of suggestion rather than that of decision. Unfortunately Pittard clings to old views where he might gain illumination by revising them; he thinks the Breton monuments Neolithic and the

French brown brachycephals Celtic. He does not refer to some survivals of ancient types (brown hyperdolichocephals) in Nor- way as studied by Bryn, but the reviewer notes with interest that there are references to dark broad heads, who are often tall, in Norway on the coast, in the Faroes, and elsewhere; these types have been found in patches of the western British and French and Spanish coasts and the recognition of them would have helped the author in his interpretations. Pittard is too wise to suppose that one can take a linguistic group

and treat it as a race, and his remarks on the Slavs in this connection are of considerable utility. He dissociates the Veddas from the peoples of S. India, though the latter clearly include an element closely akin to the Vedda; and his treatment of brachycephaly in India is not very satisfactory.

Pittard is convinced that physical anthropological data can be of considerable use in tracing movements of peoples and he is wisely cautious about opinions on race mentality. He asks us not to accept too readily even the theory of the "bellicose Nordics" and their various eruptions, but his views could have been better put after correlation with those of palæoclimatologists like O. Pettersson and C. E. P. Brooks.

That there are distinct correlations between mental and physical characters of individuals seems clear enough, but most workers will agree with Pittard in decrying hasty attempts to evaluate whole stocks morally and spiritually. Indeed, some would go much farther than he does in this direction.

It appears to be a fact that at least since the dawn of the Bronze Age, populations in our quadrant of the world, and probably elsewhere also, have included diverse types living side by side, and this may well give pause to anyone who would wish to argue on this subject.

The translator has added useful foot-notes on recent work; M. Pittard's use of literature of the last few years is slight. It is unfortunate that the spelling "peninsular" has been used repeatedly for the substantive. There is a rather unhappy element of war-mentality in several parts of the book.

H. J. F.

Malta: Archæology.

Murray.

Excavations in Malta (Percy Sladen Memorial Fund Expedition). By M. A. Murray, with a chapter by G. Caton Thompson. Part II. London, 1925. Pp. 43. xxxiii plates.

Miss Caton Thompson's contribution to this little volume deals with the Ghar Dalam cave and Miss Murray's with her excavations of the Borg en Nadur, a ruined apical "temple."

The problems of Maltese archæology present two special features among many others; the first is concerned with Palæolithic man, and the second with the megalith builders. Both are, therefore, approached in this book, but the excavators have been unfortunate, for it must be confessed that they can throw little additional light on these questions. It will be remembered that the evidence for palæolithic man in Malta rests on Mr. Despott's discovery of two taurodont teeth "in circumstances

incapable of satisfactory explanation" (I quote Miss Caton Thompson) in the cave. Nothing else has so far been discovered, in spite of careful excavation. Miss Caton Thompson has carefully reviewed all the available evidence, including an interesting discussion on the fracturing of the hippopotamus bones, which she concludes was not due to human agency. She is inclined to support Dr. Ashby in his tentative surmise that the bone deposit is "of the nature of a kitchen midden of Mousterian or Pre-Mousterian age," but admits that in spite of much careful digging no implements have been found. Unrolled hippopotamus and elephant bones are found at higher levels associated with Neolithic remains. The suggestion here made is that the bones really are contemporaneous with the bones of the breccia, but by getting lodged in crevices escaped the rolling so characteristic of the breccia bones. The whole position is from a scientific point of view unsatisfactory and it looks as if we shall have to depend for future information not on carefully organized digs but on the chance find of a peasant, a counsel perhaps of despair, but still there are other early deposits in the island, probably along nearly all the *weids*, and if a chance find were made the scientific excavator would have the advantage denied to Miss Caton Thompson of not having to wade through the work of previous diggers. Miss Murray's chapters are mainly descriptive and are beautifully and extensively illustrated—they do not include any sensational finds. The chapter on the Bronze Age Pottery is of especial value, but beyond confirming our previous knowledge adds little that is new. Archæology is like big game hunting, you do not always make a kill, and if the learned authoresses have not found their tiger, their ill fortune was assuredly due to bad luck, and we must hope for better fortune in the future.

L. H. D. B.

Tattooing.

Hambly.

The History of Tattooing and its Significance. By W. D. Hambly. H. F. & G. Witherby. London, 1925. Pp. 346, 16 plates, 16 text-figures, and map. 25s. net.

Although there are added to the title of this book the words "with some account of other forms of corporal markings," the addition is not an adequate qualification; as the author indicates in the introduction, and scrupulously carries out in the greater part of the text, "the three main varieties of body-marking, namely, painting, tattooing by puncture, and scarification have been considered simultaneously." The result is that the reader feels at times that,

instead of following a line of argument, he is involved in a maze from which there is no escape. A restriction of the study, pursued as it is "on psychological, socio-logical and historical lines," to the facts and fancies associated with tattooing, would have conduced to a directness of aim which might have given the reader a chance of appreciating more clearly the author's intentions and results. But to fire all three barrels at once suggests a certain rashness, and the sprinkling of some of the targets with small shot is not sufficient compensation for the recoil. Mr. Hambly has, in fact, been too impetuous, and has failed to attain the full reward of his diligence. Nevertheless he has brought together a great number of records of the technique and significance of body-markings, and if his conclusions are often hesitating, we are convinced that it is because he finds no grounds for dogmatism, diffusionist or other. He is not unwilling to give the Polynesians credit for the introduction of tattooing into America, and he views with sympathy the hypothesis that the practice itself originated in Ancient Egypt; but these are of the nature of acceptances on his part, rather than conclusions arising out of the results of his own investigation of the literature.

The book is divided into the following chapters: Body-marking in relation to religious beliefs and practices, Body-marking and magic, Body-marking for social and anti-social purposes, Technique of body-marking, Geographical distribution of tattooing, Historical distribution of tattooing.

H. S. H.

Papua : Ethnography.

Riley.

Among Papuan Head-hunters. An account of the Manners and Customs of the old Fly River Head-hunters, with a Description of the Secrets of the Initiation Ceremonies divulged by those who have passed through all the different Orders of the Craft, by one who spent many years in their midst. By E. Baxter Riley, F.R.A.I. London : Seeley, Service & Co., Limited. 1925. Pp. 316. 21s. net.

In this little volume, considerably abbreviated from his original manuscript, Mr. E. B. Riley has given a vivid account of the manners and customs of the Papuan Kiwai people of the Fly River, more especially as they were in evidence during the early times of his residence among them. In twenty-three years he has gained the confidence of the natives and is thus enabled to give a description of their ceremonies and beliefs from their own point of view.

The ordinary life of the individual from birth to death is described in the first half of the book. Childhood is pictured as unhappy, sad, and tearful. It culminates in the atrocities of the initiation ceremonies. The status of women and their work in the garden, the preparation of food, especially sago, and mat-making, form the subject of one chapter, whilst others are devoted to the activities of the men in house-building and canoe-making. A fully illustrated account is given of the building of the communal houses and the ceremonies attendant on the inauguration of these and the canoes. A full account is also given of the regulations and taboos relating to gardens, and dugong and turtle fishing.

In the second half of the book, Mr. Riley describes the sacred and secular dances and ceremonies of the Kiwai. Some of these he himself saw, but for others he had to rely upon the accounts given to him by natives who had themselves passed through the ceremonies. A chapter on the theory and practice of head-hunting follows, with a narrative of an actual raid by one of the participants. The book concludes with an account of sorcery and minor superstitions, and Kiwai ideas of the spirit world.

There are about fifty illustrations from photographs and native drawings, two maps, and an index. Mr. Riley's work adds considerably to our knowledge of an extremely interesting people, who are probably destined to become in the near future considerably influenced by their proximity to Australia, and whose way of life is rapidly changing. Mr. Riley's additional records, especially his grammar and dictionary of the language, and collection of texts, ought soon to be made available for students.

S. H. RAY.

Africa, West : Ethnography.

Meek.

The Northern Tribes of Nigeria. By C. K. Meek, B.A. Oxford University Press. Pp. xviii + 312 + 277. Maps.

Mr. Meek has attempted the gigantic task of giving in the limited space of two volumes a systematic ethnographical survey of the three hundred tribes inhabiting the Northern Provinces of Nigeria. He is to be warmly congratulated on his successful achievement. He has produced a book which for many years to come will be the standard work on Northern Nigeria. It is not meant to supplant the valuable compilation of Mrs. Temple; the author has drawn largely on it himself and in arranging its immense material enriched it with a wealth of observations made by himself during his labours as census officer. Under the influence of different cultures and the newly established order of European rule,

many tribes are in a state of transition. From the ravages of the disintegrating forces of foreign conquests and the slave trade which subjected, broke up, or destroyed community after community; from the unifying efforts of Islam and a superior civilisation tending towards the formation of new kingdoms and new nations—Mr. Meek has disentangled the ethnical components and presented them to us in logical sequence.

We are so deeply in debt to him that it seems ungracious to find fault with his views; yet I venture the opinion that, in refusing to give us his conclusions concerning the past existence of mother-right he errs on the side of overcautiousness. Besides the Hona and Mbula mentioned by him, the Vere are said to be still reckoning their descent matrilineally. And if he can find no clear case of matrilineal residence he might consider the Kugamma practice which permits a man to marry his own first cousin, while debarring him from any union with his wife's relations, however distant. To me this seems to indicate a complete absorption into the wife's family or clan.

A subject deserving more space than Mr. Meek could spare for it is the artistic side of the people's life. The scanty information he gives us about their musical abilities refers to instrumental music only; and he tells us nothing of the dramatic dances observed by Caillié; the plastic arts are passed over in silence except for mentioning some acquaintance with the *cire-perdue* process. But if this excellent book, which is worthily published and profusely illustrated, gets its deserts, he will have plenty of opportunities of filling these gaps in future editions.

E. T.

History.

Newton.

Travel and Travellers of the Middle Ages. Edited by A. P. Newton, **111** M.A., D.Litt., F.S.A. History of Civilisation Series. London: Kegan Paul, 1926. Pp. vi, 223. Price 12s. 6d. net.

The substance of this volume was comprised in a course of public lectures delivered at King's College in the Autumn Term of 1925, although some of the contributions have been rewritten and much new material added. In many respects the volume is of as much interest to the anthropologist as to the geographer. Study of the history of exploration, especially in the Middle Ages, is invaluable in throwing light upon questions relating to trade routes, knowledge of which is essential in considering problems of distribution. In this connection attention may be called in particular to the chapters on "Trade and

Communication in Eastern Europe," by Baron A. F. Meyendorff, and "The Opening of the Land Routes to Cathay," by Dr. Eileen Power, while Professor T. W. Arnold's account of Arab Travellers serves to emphasise the wide extent of the field which was covered in these early days. Professor Alan Mawer's account of the Vikings is especially useful as a corrective of popular misconceptions. The folklorist will wish that the editor had allowed himself more space to deal with "Travellers' Tales." The size of the book precludes anything more than a very summary treatment, but so far as it goes it will serve as a useful introduction and guide to the most recent results in a fascinating subject.

E. N. F.

Palæanthropology.

Verneau.

Les Origines de l'Humanité. Par **112** R. Verneau. Paris: Rieder, 1926. Pp. 80 and 59 plates. 20 × 16 cm. 15 fcs.

A small work, richly provided with illustrations, and giving a popular exposition of the author's views concerning the early history of mankind. He discusses Neanderthal, Grimaldi negroid, Crô-Magnon and Chancelade races for the Palæolithic, after a brief mention of the Piltdown skull; one doubts whether this classification satisfies anyone nowadays. The Solutrean culture is treated as having an epoch of its own, and this again is an opinion that seems to be passing away. These points illustrate a current tendency to a marked conservatism which would be better justified if it were due to the desire to avoid speculative complications. The illustrations are interestingly chosen for their purpose of gaining the layman's attention.

H. J. F.

Italy: Archæology.

Bryan.

Italic Hut-Urns and Hut-Urn Cemeteries: a Study of the Early Iron Age in Latium. By W. R. Bryan. Papers and Monographs of the American Academy at Rome. Vol. IV. 1925. Pp. 204 and 11 plates. **113**

The conclusions of this interesting and useful study are directly opposed to those of Dr. Randall-MacIver on two vital points. Not only does Mr. Bryan refuse to recognise any trace of "Villanovism" in the Latian "necropolises" (*sic*). He further argues that the hut urns from cemeteries in Etruria represent a survival of an older element in the population that had there been overlaid by Villanovans. In fact for him hut urns are essentially monuments of Latins. On the first point he seems to

have proved his case. The second argument would have been more convincing could it have been shown that the hut urn burials at Vetulonia and Tarquinii are older than the undeniably Villanovan interments or are at least linked on to older pre-Benacci material from the same sites.

Considering the undoubted points of contact between English and American, the translation of this work is not so easy as might be expected. We may gloss: hypothecation = supposition, provenience = provenance, deposition = act of placing, location = site. V. G. C.

PROCEEDINGS OF SOCIETIES.

British Association.

Section H. Anthropology.

Report of the Proceedings of Section H of the British Association, Oxford Meeting, August 4-11, 1926. **114**

At the annual meeting of the British Association held at Oxford from August 4 to 11, Section H (Anthropology) met at Mansfield College, under the Presidency of PROFESSOR H. J. FLEURE, who took as the subject of his Presidential Address "The Regional Balance of Racial Evolution," surveying what is known and thought as to how, when and where the evolution of modern man worked itself out. No attempt can be made here to summarise PROFESSOR FLEURE's argument, which, surveying the subject from all sides, was itself necessarily much condensed. He himself summed it up by saying that he suggested that the development of the individual depended on hereditary factors of a conservative nature and on environmental influences which have changed with climate, food and equipment, and so have affected plastic infancy and in the end have moulded race types, blending hereditary characters, sometimes brought from afar, with other features in which the changes of environment have had more power. He assigned a somewhat limited value to taxonomic treatment of the question; but thought rather of regional gatherings together of physical characters. The address will appear in full in the Annual Report of the Association.

Apart from the Presidential Address, the programme of the sectional proceedings was of unusual interest. It is certainly many years since so large a number of communications of importance and so uniformly high in quality has been presented to the Section.

Among the archaeological communications, which again formed the larger proportion of the sectional programme, Miss GARROD's account of the human skull associated with Mousterian implements found at Gibraltar had been awaited with considerable interest since the first announcement of its discovery at the Devil's Tower on June 11 of this year. The conditions of the discovery of a frontal and parietal bone in a bed of hard travertine

forming the fourth in a series of five implement-bearing deposits in the cave, place its age as Mousterian beyond question; while MR. DUDLEY BUXTON's description of its physical characters, tentative as it was, showed that it was unquestionably of Neanderthal type. The absence of prominent brow ridges and other features indicated that it was immature, though of indeterminate age and sex, SIR W. BOYD DAWKINS's survey of the range of *Anthropus Neanderthalensis* on the Pleistocene continent formed a fitting pendant to Miss GARROD's paper. SIR W. BOYD DAWKINS also contributed a paper on the cult of the Neolithic axe, suggesting that the highly polished stone axes of the burial mounds of France, Germany and Britain were probably intended for ritualistic and not practical use. A paper which gave rise to some considerable discussion was that by Miss LAYARD on a provincial Magdalenian flint industry from the Colne Valley, Essex, in illustration of which a series of finely worked flint implements was exhibited. MR. LESLIE ARMSTRONG described his excavations in the Pin-hole Cave, Cresswell Crags. Hunan artefacts of Aurignacian, Proto-Solutrean, Upper and Lower Mousterian types were found, as well as lance points of reindeer antler, from the old Mousterian level, believed to be new to science. The fauna is rich and pleistocene in character. A paper by MR. GORDON CHILDE on the Terramare of the Hungarian Bronze Age, pointed out the need for the revision of the view that the *Terramaricoli* were to be derived from the Middle Danube Valley, and suggested that their ancestors might be a mixture between the corded-ware makers and the lake-dwellers from the Eastern Alps.

Recent work in Mediterranean and Mesopotamian archaeology was well represented. SIR ARTHUR EVANS dealt with the shaft graves of Mycenæ and their contents in relation to the Beehive Tombs, and showed that recent discoveries at Knossos demonstrated that the theory which assigned a relatively late date to the shaft graves was untenable. MR. W. L. CUTTLE was deputed by MR. A. M. WOODWARD, Director of the British School of Archaeology at Athens, to describe the

work of excavation during the past three years in the Theatre and Acropolis at Sparta; and Mr. W. A. HEURTLEY, Assistant Director, described the excavations of the School on sites in Macedonia.

The excavations of the joint British Museum and University of Pennsylvania Expedition to Ur during the past season were described by Mr. C. LEONARD WOOLLEY. Although a stratified succession of constructions of unknown date has been encountered in excavating to a depth of 36 feet below the floors of a building put up in 2100 B.C., which are the earliest yet found at Ur, they still belong to a metal-using age, and the level of the Painted Pottery of Al-Ubaid has not yet been reached. PROFESSOR LANGDON described the results obtained by the Oxford-Field Museum expedition to Kish in 1925-6. Investigations were confined to two periods—the Early Sumerian and the Late Babylonian. The existence of painted pottery at Kish as late as 3000 B.C. is now proved. It would appear that Sumerian civilisation is older than the Elamite. MR. DUDLEY BUXTON dealt with the physical character of the early population from the Sumerian and Babylonian strata. Measurements on the modern inhabitants showed that they were essentially the same as their predecessors of 5,000 years ago. MR. R. CAMPBELL THOMPSON described the scientific knowledge of the Assyrians in the seventh century B.C.

One of the most interesting and important discussions of the whole meeting took place in an afternoon session devoted to the question of the early culture of Egypt. PROFESSOR SIR FLINDERS PETRIE again put forward the suggestion as to the Caucasian origin of the Badarian civilisation, which he regards as Solutrean. This was followed by two papers, one by Miss E. W. GARDNER dealing with the geology of the Fayum, and another by Miss G. CATON-THOMPSON on the early culture of that area, each paper embodying the results of recent work. The flint found in the Fayum, and in the oldest stratum at Badari, Upper Egypt, is regarded by Miss CATON-THOMPSON as a result of her investigations of its culture status and relative date by the systematic excavation of habitation mounds, as of an advanced Neolithic type and not as Solutrean, as held by Sir Flinders Petrie.

MRS. ZELIA NUTTALL'S study of the ancient calendar systems of America was a communication of considerable importance, particularly in its bearing upon the question of diffusion and the independence of origin of the calendar.

Some important communications in physical anthropology were offered: DR. R. T. GUNTHER on the hairlessness of man—a condition which he suggested was related to the use of fire; Miss M.-E. B. RUSSELL, on the distribution of human hair form; DR. SHAXBY on the relations of skin and light; Miss FLEMING on the study of growth in children, giving further results of her consecutive observations; and MR. E. G. BOWEN on anthropological types and tuberculosis—a paper which attracted a good deal of attention. A discussion on physical and mental aspects of heredity in a joint session of the Psychological, Zoological and Anthropological Sections was opened by DR. C. S. MYERS for the psychologists, PROFESSOR RUGGLES-GATES for the anthropologists, and PROFESSOR JULIAN HUXLEY for the zoologists.

Ethnography and Ethnology were well, if not numerously, represented. CAPTAIN G. PITT-RIVERS dealt vigorously with the question of depopulation in the Pacific, deprecating views which regard depopulation as inherent in certain native customs; by Miss BLACKMAN, who gave an account of her work among the peasant population of Egypt, especially in connection with beliefs relating to saints and touching on their ancient analogies; Miss SIMPSON dealt with the effect of the geological and geographical conditions as affecting settlement on the Wychwood village sites; and Miss C. BUTLER described the work in "local lore" of some Oxfordshire schools. MR. WILFRED BONSOR dealt with the "Elfshot" belief, this being perhaps most familiar in the popular connection with the neolithic arrow-head.

An important discussion took place in a joint discussion with the Geographical Section on the effect on African native races of contact with European civilisation. This arose directly out of the Presidential address of MR. ORMSBY-GORE to the Geographical Section, and was opened on behalf of the anthropologists by MR. H. J. E. PEAKE and the REV. E. SMITH. SIR FREDERICK LUGARD, CAPTAIN PITT-RIVERS, MR. HUGH WYNDHAM, MR. HOBLEY and MAJOR CHURCH were among the speakers who followed. It is hoped that the subject may be further discussed at a meeting of the Institute in the coming Session.

At a special meeting of the section held on the evening of Saturday, August 7th, Miss VIOLET ALFORD read a paper on the Ritual Dance, which was illustrated by a side of dancers from Letchworth. This, it is also hoped, may be repeated at a meeting of the Institute.



Photo by J Palmer Clarke

William Ridgeway

ORIGINAL ARTICLES.

With Plate L.

Obituary.

Myres: Haddon.

Sir William Ridgeway. August 6, 1853—August 12, 1926. By Professor J. L. Myres, M.A., F.B.A. With Plate L. **115**

The death of Sir William Ridgeway removes from among us a picturesque figure, a stimulating teacher, and an original and fertile worker. Born in King's County of an old fighting stock, *ipsis Hibernis Hibernior*, most of all while protesting that he had nothing to do with them, he went to Cambridge from Trinity College, Dublin, with a fine record of scholarship in ancient and modern literatures, graduated from Gonville and Caius College with high honours in 1880, and was elected in due course to a fellowship there. It had been his not unreasonable expectation that a teaching post would be found for him at Cambridge, but colleges, like china-shops, expect, and indeed require, a technique of handling which does not come naturally to everyone. Ridgeway had his own technique—in research, in controversy, above all in teaching. "I trail my coat before the young men till they learn to hit me," he would say, "and I like them to hit me hard": and certainly the game of treading on Ridgeway's coat was the best sport in the world, if (like the total immersion which usually followed) "the child shall well endure it."

In 1883 Ridgeway was appointed to the Chair of Greek in University College, Cork. Here, though it was his mood to call it exile, he was in his element, teaching classics in his own inimitable way, composing topical Greek plays for student performance, linking class-work with his own researches and current controversy in vivid digression, writing copiously on every department of humane studies, a staunch friend to the highest interests in Irish education, and during vacation residence—not indeed in Cambridge, but in a delightful old house and garden a mile or two out of town—an acute critic of academical matters there, and one of the founders of a distinguished school of archæological and anthropological workers. His own output during this period was voluminous, but discursive; he was amassing stores of information, which his lively intellect and scholarly fancy put to the most unexpected and unsettling uses. Wherever he descended, there was a stirring of the waters; and it is no reflection on his work if he raised more questions than he solved.

His appointment to the Disney Chair of Archæology in Cambridge, in 1892, followed by a Fellowship at his old College, and the Brereton Readership in Classics restored him wholly to the surroundings most congenial to him early enough for his versatile energy to set its mark on the history and the politics of the place. Professing unqualified conservatism on such current problems as the admission of women and Greek-less persons to the University, and ruthless in his handling of them, he will be remembered more justly as a fearless and persuasive advocate of far-reaching innovations in the traditional outlook of the University on the "humanities." The scientific study of archæology was clearly distinguished in his mind from the department of æsthetic criticism with which it has sometimes been confused, to the detriment of both those disciplines; and the services which archæological study is able to render both to literary interpretation and to systematic philology were the more convincingly represented by one who was both a fine scholar and a man who had begun to make his mark in philology before these other interests claimed his chief devotion.

Both in archæology, therefore, and in the newer subjects of anthropology and ethnography, the liberal provision now made at Cambridge for advanced study owes much to Ridgeway's advocacy, as well as to his outspoken criticism. But most of all have they been indebted to his personal influence as a teacher and guide of

students. His quiet home at Fen Ditton became, and remained to the end, a place of pilgrimage for travellers, excavators, colleagues in each of his manifold enquiries, as well as for generations of undergraduate friends. If you had to wait for your own talk with Ridgeway, there were sure to be men there whom you had wanted the chance to meet; and there was a gracious presence quietly making everyone most truly at home. The house was a library, a museum, a work-shop. Ridgeway talked best when he was handling one of his many treasures; and even on a journey was seldom without them. On one occasion, at least, the succession of gems and precious stones which he produced from his pockets in the train roused the suspicion of fellow-travellers as to how he had come by them; for his unusual appearance, and abrupt manner of speech, did not at first sight betray the Cambridge professor.

In middle life, Ridgeway took his full share in the work of the London societies interested in his various subjects—his services to the Royal Anthropological Institute are too well known to readers of this journal to need description—and his knighthood was well deserved recognition of work none the less public in its importance, in that it was carried on by personal discussion and private correspondence. His introduction of a deputation to Mr. Asquith as Prime Minister, urging greater facilities for anthropological training and research for civil servants abroad, was skilful and dignified advocacy of a subject very near his heart; and there were many occasions of that kind.

Difficulties of eyesight, which had probably been apparent to bystanders even before they were a serious handicap to Ridgeway himself, gradually restricted the range of his own reading, and made him more dependent on the newer information of his visitors; certainly not diminishing the variety and interest of his own talk, or of the friendly discussions of which he remained the centre. How many fresh notions, or queer bits of knowledge, have been put in suspense till we could discover "what Ridgeway will say about it!"

Among his more systematic writings, the most significant, at the time of its appearance, is probably his "Origin of Metallic Currency and Weight Standards", published in 1892, the year of his Cambridge appointment. It combines careful metrological researches, in which his interest was lifelong, with those illuminating comparisons between classical and non-European data, in which he delighted, while he was seldom happier than when dispelling other people's illusions about such arguments. It marked a turning point in the interpretation of ancient measures of weight, and (still more) of value, even more by the width of its outlook, and the robust common-sense of its treatment, than by the permanence of the particular conclusions to which his own study of the data led him.

Of his "Early Age of Greece," the first volume appeared in 1902. It was a restatement of theories which he had advanced some years before, and his fuller treatment of them was unduly influenced by an unfortunate controversy over his original article in the *Journal of Hellenic Studies*: still more was his reception of the criticisms, which the book itself provoked, animated by the wish, quite frankly expressed, to "show up those other fellows" in the style of which he was a master. It was indeed current jest at the time that "the second volume would be the third," if it ever appeared—so voluminous were his drafts for the preface to it, in which opponents were to be "justified," in the Scottish sense of the term. But no second volume came, for all the proof-corrector's toil: and in great part because the first had "advanced knowledge" in so many directions by the shrewd questions and bold conjectures to which it was for others to supply answer and the test of positive discoveries.

Instead, five years afterwards, we had the "Origin and Influence of the Thoroughbred Horse," not at first sight a topic relevant to the origin of the Homeric Achæans whose doings had dominated the "Early Age of Greece." But

one of Ridgeway's early essays had dealt with the stature and other peculiarities of Homeric horses, and he was himself—as befitted his antecedents—as much at home in the stud-farm, as in the coin-room of the British Museum; and while he castigated the vagaries of the comparative method in others, he saw no inconsistency in the careful observation of ducks at Fen Ditton, as a supplement to Darwinian literature. That his arguments proved equally clearly that Egyptian horses were pea-green—as one of his reviewers put it—troubled him not at all; for his book was received with equal delight by lovers of the horse who read no Greek and lovers of Homer who knew little about horses.

In the same year, 1907, he turned from breeds of horses to strains of human descent, in a memorable essay "Who were the Romans?" which restated older arguments for a composite origin in the light of Conway's researches among Italic dialects and the new anthropological treatment of ancient skulls and portraits. It was the counterpart to his argument about the origin of the Greeks, and a serious contribution to Mediterranean ethnography.

In quite another field, at first sight, his "Origin of Greek Tragedy" (1910), and its sequel "The Dramas and Dramatic Dances of Non-European Races," put forward exceedingly suggestive and provocative views both about the mature drama of historic Greece, and (once again) about the significance of its peculiar conventions, in relation to that ever-insistent problem of the composition of the Greek people.

In spite of increasing infirmities, Ridgeway remained amazingly active to the end. The death of his devoted wife early in the present year did not prevent him from preparing for the meeting of the British Association—a favourite battleground with him—a paper on the "Origin of the Scottish Race" and he intended to present it in person. But at the last moment the visit was cancelled, and on the day following that on which the paper was to have been read, the end came suddenly, of a strenuous, inspiring, whole-hearted pursuit of "humanity," in the finest sense of that word, by a man of many interests, many pupils, and many friends; a memorable figure in his own University, and a stimulating force wherever his work was known.

JOHN L. MYRES.

Sir William Ridgeway. *By A. C. Haddon, Sc.D., F.R.S.*

By the death of Professor Sir William Ridgeway the University of Cambridge loses one of its most prominent and illustrious men. His was a striking personality, both physically and mentally, and though for many years increasing imperfection of eyesight rendered his gait halting, his mental energy was wonderfully maintained, and his joy in life and in controversy was scarcely diminished until the sudden death of his beloved and devoted wife on the 29th of last May. As was to be expected, he bore his tragic loss most heroically, but it was evident to his friends that he was a sore stricken man, though on occasion traces of his old energy flared up.

Ridgeway was a man of strong affections and equally strong aversions, and he expressed himself accordingly. There were some who could not make allowances for his temperament, but most could and did so, recognising that beneath all disagreements there was a burning zeal for the cause or opinion that he believed to be the right one. This zeal made him a warm and energetic friend, who never spared himself to help another—but also a thoroughgoing adversary.

It is for others to speak of Ridgeway's brilliant attainments in purely classical studies, but it was evident that he was increasingly attracted towards the more human aspects of classics. His interests in classical and prehistoric archæology were very wide and he was no mean antiquary. In order to elucidate various problems and specific points in classics and archæology he turned his attention to

ethnology, certain aspects of which made a great appeal to him; besides reading widely in that subject he came into close touch with civil servants, missionaries and others who gave him information from many parts of the world. He was an excellent correspondent, as many can testify, and by the infectious enthusiasm of his conversation and his letters he did a great deal to awaken or maintain an interest in ethnological problems among a wide circle of friends.

Ridgeway's concern for anthropology manifested itself in University affairs. It was due to his initiative and driving force that a Lectureship in Ethnology was instituted in May, 1900, and a Readership in the same subject in June, 1909. It was also largely through his efforts that a Board of Anthropological Studies was established in May, 1904; and thus the study of anthropology became officially recognised in the University. His energy and deep knowledge of University procedure and politics were continuously exhibited at the meetings of the Board, and he undoubtedly had a great share in establishing anthropology firmly as an academic study. About twenty-five years ago he founded the Cambridge Anthropological Club, which at first was practically confined to graduate members of the University, but, as students began to study anthropology, the Club has become wider in its scope.

The Museum of Archæology and of Ethnology has always been very close to Ridgeway's heart, and in innumerable ways he has worked for its welfare. It was due to his efforts that the Murray Collection of Irish Archæology was obtained for the Museum, and, indeed, the same may be said for other collections and specimens. Besides a large number of books and pamphlets, he has bequeathed to the Museum his very varied collection of archæological and ethnological specimens, all of which are of interest and many are of considerable value; particular mention should be made of his valuable collection of currency, which he amassed when compiling his "Origin of Metallic Currency and Weight Standards," though many specimens have been acquired since that book was published in 1892.

He was the contributor of many communications to the Cambridge Antiquarian Society and very frequently joined in the discussions of papers in his usual lively and informing manner. He and Lady Ridgeway greatly enjoyed the excursions of the Society and contributed in great measure to their success.

On the 31st of July, 1913, a large number of friends, colleagues and students of Sir William gave him a dinner in Gonville and Caius College, and presented him with a volume of "Essays and Studies" to commemorate his sixtieth birthday on August 6, and also to express their personal friendship to him and their appreciation of the deep obligation of many branches of learning to his erudition.

Cambridge will be a duller place now that he is no longer among us, though we are all thankful that his end was swift and painless and that it saved him from increasing infirmities and a decreasing grasp of affairs. A. C. HADDON.

Archæology: Ice Age.

Breuil.

Palæolithic Industries from the beginning of the Rissian to the beginning of the Würmian Glaciation. By M. l'Abbé Breuil. **116**

The existence has long been recognised of a great group of industries, habitually considered as running parallel with the Mousterian of the caves, but wrongly, since it is in fact anterior to the latter. M. Commont has established the existence in the Muchembled pit at Montières, near Amiens, of an industry marked by blades and small Levallois flakes, which is associated with a fauna of *Elephas antiquus* in clays and gravels at a very low altitude above the River Somme. Its technique, with prepared striking-platform, is already that of the Mousterian, but in time it belongs to the last interglacial epoch (Riss-Würm).

A nearly identical industry exists at Crayford, in the Thames valley, superposed on the gravels of the Low Terrace in a clay with cold fauna, above which lies a *Corbicula* bed belonging to the same warm stage as the above-mentioned level at Montières. A second cold level overlies the *Corbicula* bed. In this case the industry is clearly anterior to the last interglacial phase, and consequently of Rissian age.

At Baker's Hole, Northfleet, an industry of large heavy Levallois flakes, associated with a cold fauna, exists *in situ* on a plateau at 50 ft. O.D.* It underlies a deposit of chalky sludge (Coombe Rock) of glacial origin, or, more exactly, contemporary with a glaciation. The industry appears again in a derived state in the Coombe Rock itself, the angles of the flakes broken in rather a special way by the action of this deposit. It appears to be anterior, morphologically speaking, to the Crayford industry, and therefore probably anterior to the Rissian maximum.

Messrs. R. A. Smith and Dewey have found implements of Northfleet type in the gravels and sands of a terrace 120 ft. above the River Stour at Sturry, Kent,† overlying an Upper Acheulean level and a more ancient gravel containing Acheulean ovates.

At Montières the very low levels contain an industry similar to that of Baker's Hole and broken in the same way, localised in gravels heavily charged with chalk, which have no doubt been formed by the washing of deposits of the same nature as the Coombe Rock. As they approach the upper part of the Low Terrace these gravels pass underneath clayey and sandy beds containing an industry of blades and small Levallois flakes of the type described by Commont and similar to the Crayford industry. Some of these are slightly broken by ice, others are intact. A molar of *Elephas antiquus* has been found in the clay. The massive Levallois industry of the chalky gravels would, therefore, be of Rissian age, as at Baker's Hole.

In the pits at Montières which lie between the mainroad and the path to Étouvy, the recent loess or *ergeron* overlies the group of deposits described above, and contains a more recent industry of Levallois flakes and blades, corresponding to one or other of the levels described by Commont at St. Acheul and elsewhere, and which belong to the last glaciation.

In the sites studied by Commont the group of industries contained in the recent loess is immediately preceded by a level of cordiform hand-axes associated with light Levallois flakes and blades, bearing a close relation to the level found at the base of the Mousterian in the caves (which is marked by an almost complete absence of reindeer and a generally temperate fauna). These same cordiform hand-axes are found again, as M. Boule has shown, in Riss-Würm deposits near Aurillac and in the Ain. They appear in England at the same level in various sites, especially at Warren Hill and High Lodge, Suffolk, and at West Runton, Norfolk. They belong, therefore, to the end of the last interglacial phase.

It results from these facts that a whole cycle of industries of Levallois type, which might well be baptised Levalloisian or Montierian, occupied the whole of Rissian and Riss-Würmian times.

A clear relationship exists between this cycle and the industry known in Belgium as Mesvinian, in which the same preparation of the striking-platform is visible, but in which the resulting blades and flakes are less voluminous, less well-

* The Baker's Hole site does not in my opinion belong to the 50-ft. terrace, but to a plateau which immediately dominates it, since at one point I saw fluvial gravels and sands banked against the edge of this plateau.

† Henry Dewey and Reginald A. Smith: "Flints from the Sturry Gravels, Kent." *Archæologia*, 1925, LXXIV, p. 117.

prepared and more irregular. It is not improbable that the Mesvinian represents the most primitive facies of the whole cycle.*

Another question that suggests itself is that of the relation of the Levalloisian to the industries with lanceolated or cordiform hand-axes of the types of La Micoque and Combe-Capelle. I have already pointed out that the cordiform type is found at the base of the *ergeron* or recent loess, and also at a slightly higher level, associated with developed Levalloisian forms, and that in England it occurs at a level corresponding with the end of the last interglacial phase. The industry with highly-evolved lanceolate types similar to those of La Micoque is found at St. Acheul beneath the bed of angular gravel at the base of the *ergeron*, and therefore belongs to a slightly earlier moment of the last interglacial phase than the industry of Combe-Capelle type. In England the Micoquian has been found in place in the Traveller's Rest pit at Cambridge, above the remains of an old boulder-clay at approximately the same level as a slightly-derived Levallois flake of early type. Above it, in places, were visible vestiges of the Upper or Würmian boulder-clay. Several Micoquian pieces have been found at Wolvercote near Oxford at the bottom of a buried channel, associated with a temperate fauna. In the higher levels of the channel the fauna is cold, and several Mousterian flakes have been found. The evidence for the age of the Micoquian is, therefore, the same as at Cambridge and in France.

In the Muchembled pit at Montières very fine Micoquian points were found by Commont both above and below his evolved Levalloisian industry with warm fauna. The older ones were found in an angular gravel lying at the base of certain vestiges of an ancient loess, and were, therefore, of Rissian age, as the cold fauna clearly showed; the others occurred in a clay containing warm shells, overlying the level of the Levalloisian with warm fauna. It, therefore, becomes necessary to postulate a coming-and-going of the peoples who made the Micoquian hand-axe, and to admit that the evolved Acheulean interrupts from time to time the development of the Levalloisian, up till the moment, corresponding to the base of the first recent loess, when the two cultures appear to fuse in the Combe-Capelle industry, associated, as in many of the cave-sites of this level, with an incipient cold fauna.

It seems to me, therefore, that a too-simple formula is inadequate to express the industrial evolution of the period which extends from the beginning of the Rissian glaciation to the beginning of the Würmian. Several cultural streams follow and replace each other, and their final fusion occurs only at a comparatively late date.

A further indication of the complexity of the industrial evolution of the last interglacial phase is furnished by the facts observed at High Lodge, Mildenhall. We there find, sandwiched between the two latest boulder-clays of East Anglia, two industrial levels, of which the more recent yields the cordiform hand-axes already mentioned, while the older is characterised by flakes with large unprepared striking-platforms, very skilfully retouched along the edges. In spite of the presence of points and of side-scrappers in abundance, this industry differs widely in technique from the Levalloisian or the Mousterian proper, being more closely related to the flake industry found in the lower levels of the Barnfield pit, Swanscombe (100 ft. terrace of the Thames). In age it can be only slightly later than the Rissian

* As I am responsible for the use of the word Mesvinian by Mr. Hazzledine Warren to denote his industry from Clacton-on-Sea, I feel obliged to correct my terminology. As a matter of fact the whole group originally called Mesvinian in Belgium should be divided into two lots, differing from one another in geological position and in technique. The more recent of the two, showing prepared striking-platforms and tortoise-cores, can alone be properly called Mesvinian; the other, which is more ancient and does not show preparation of the striking-platform, has been wrongly identified with it. It is with the latter, more ancient industry alone that the implements from Clacton can be compared.

maximum. The glacial gravels which underlie the brick-earth containing the High Lodge industry, and which are well exposed at Warren Hill, three-quarters of a mile away, contain in a very derived state all the industries anterior to the Middle Acheulean, including the level characterised by large ovates. The extremely worn condition of the oldest specimens bears witness to a long passage through a succession of gravels or glacial deposits. These facts confirm those observed at Cromer, where, apart from the undoubtedly human industry of the foreshore, which is older than the Forest Bed, a Chellean implement, very slightly derived, was found at the very base of the Till, while a completely unworn cordiform hand-axe, similar to those from High Lodge, lay above the Contorted Drift beneath an angular gravel which is probably of recent glacial origin (cf. the Trail).

This distribution of industries in the English glacial deposits agrees with the results which I have obtained in the valley of the Garonne, where the oldest industries are found in a derived state in the gravels of the 60 m. terrace, this terrace being shown by M. Déperet to be in relation with the Mindelian moraines of the region.

H. BREUIL.

Ethnology.

Thomson.

Ancient History in Keane's "Man, Past and Present" (1920). By **117**
J. Oliver Thomson.

In this book, "revised and largely re-written" by A. H. Quiggin and A. C. Haddon, the use made of classical writers is sometimes hardly worthy of a work of such importance.

P. 304. The authors regret that E. H. Parker lent the weight of his authority to the statement that the word "Türkö" [Turki] goes no farther back than the fifth century A.D., and they produce "Turki tribes bearing this national name already "seated on the Tanais (Don) about the new era." For this they quote Mela and Pliny (whose reference should read vi. 7. 1) as mentioning Turcæ or Tyrcæ, and even believe that these variants "are noteworthy as indicating the same vacillating sound "of the root vowel (*u* and *y* = *ü*) that still persists." This is a mare's nest: there is no significance in these trifling variants, and both are palpably a false reading for the Iyræ of Herodotus IV 22. The farrago of barbarous names which both these Latin writers mistake for geography was peculiarly liable to corruption; at the best it is possible that Turcæ (or Tyrcæ) was deliberately substituted by mediæval copyists who knew of Turks. Both are dangerous authorities for ethnology, unless their disparate sources can be sorted out. The passages concerned are, with some alien matter, a garbled version of the series of peoples beyond the Don given by Hdt. IV 21-7, 108-110, 123-4. There the Thyssagetæ and Iyræ appear as hunting tribes in a wooded region, and so Mela paraphrases. Whatever were their affinities (Ugrian, Finnish?), it is not seriously proposed to alter Herodotus's text and find Turks in the fifth century B.C. That the authors ignore Herodotus here is the more curious, as they quote him (p. 536) for the Sauromatæ and Budini of the same series.*

P. 305. If any meaning of value is to be attached to "Turki," it is an anachronism to make the Parthians of 250 B.C. "belong to the same connection." All nomads are not Turks. Arsaces came with a band of the very Dahæ so dubiously discussed on the next page.† Rémusat's equation of these with the Ta-Hia of

* With a strange mistranslation. Read "no trees cultivated or wild" (iv. 21). "Seven distinct dialects" does not truly represent iv. 24. Scythians who go as far as the Argippæi have to use seven interpreters.

† Such remarks as "possibly the Dehavites of Ezra" are to be deprecated in a serious work.

Chinese records now meets with little favour: the Dahæ were not in Bactria. The Chinese name has not been satisfactorily explained: Marquart's equation with Tochari, accepted from the Chinese side by Chavannes, deserves mention but raises many difficulties. The references of Strabo (511) and Justin (Prol. XLI-II) to Tochari and other invaders of Bactria have acquired great importance from the discovery of "Tocharish" (see p. 441).

P. 310. It is very doubtful if the "Onoi" [Οὔνοι] of Dionysius Periegetes are the "On-Uigurs (Ten Uigurs)." Others take them to be early Huns. But the text is doubtful. The word occurs amid a jumble of names suggesting Strabo 514 (from Eratosthenes), who has Οὐρίους (*cp.* Plin. Udinos; Tab. Peut. Utios; Bunbury, "Hist. Anc. Geogr.," II 480).

P. 543. The name Dadikæ (not Dadikes) in Hdt. III 91 has not yet been explained. The identification with Tajiks is very dubious, and the footnote is wild, "Even Ptolemy's Πάσιχαι appear to be the same people, π being an error for τ, so "that Τάσικαι would be the nearest possible Greek transcription of Tajik." (Do the authors refer to the Πάσικαι of Sogdiana, VI 12-4?)

P. 241. J. T. Last is quoted, without protest, for Madagascar being the island of Menuthias, "described by Arrian in the third century A.D." The accidental ascription to Arrian of the Periplus of the Erythræan Sea, an anonymous work of the first century, has long been abandoned. Its editors generally make the island Zanzibar or Pemba or a fusion of both.

P. 470. The "Sidonian Phœnicians" are made to found Carthage and Utica, probably about 1500 B.C. The ancient authorities, such as they are, mostly put Carthage in the ninth century, and suggest no date earlier than the twelfth for either. Even this date is suspect to the archæologists, with whom the Phœnicians are under a cloud.

To pass to other ground, it is surely wild to suggest that ancient Sumer was in touch with China by sea (p. 207), and it is misleading to assert (p. 210) that, in Chinese records, "the first certain date would appear to be that of Yau, first of the "Chinese sages and reformer of the calendar, 2357 B.C." The Chinese Herodotus, Se-ma Ts'ien (145-87 B.C.), refrains from any exact chronology before 841. Hirth ("Enc. Brit.," Chinese Origins) and other Sinologists, while attaching some value to legends of earlier times, find the first fixed date in an eclipse of 776 B.C., during the Chou dynasty.

J. O. THOMSON.

Tibet: Religion.

Rybot.

A Small-Pox Edict Pillar at Lhasa. By Major N. V. L. Rybot, D.S.O. **118**

The accompanying illustration is from a drawing of the Small-Pox Edict Pillar which stands in Lhasa, opposite the main entrance to the *Jo-khang* or cathedral. The face of the slab, upon which the cup-marks or pock-marks were made, originally bore the Chinese characters of the Edict. The slab is about 6 feet high, and of granite or some kindred igneous rock. Down the centre of the slab is a vertical row of cup-marks shaped like spoons, one overlapping the other. Some of the larger cup-marks on either side of this vertical row are also spoon-shaped; but the others approximate to the circle.

My sketch does not attempt to give an accurate and scientific map of the numbers and relative positions of the marks, but it gives a true impression of the monument as a whole.

Tibet is subject to deadly epidemics of small-pox, and it was of this disease that a famous Tashi Lama died when on a visit to Peking in 1780.

His monument in the Lamaist Temple just outside the north gate of Peking is a veritable marble wonder.

If, as is probable, the Lhasa monument was erected soon after this Tashi Lama's death, I would seek confirmation of this statement in a comparison between the marble work of the two monuments. The Edict slab is based on, as well as crowned by, elaborately sculptured marble blocks—evidently the work of Chinese craftsmen. The tile-work is also Chinese. The cup- or pock-marks, then, have been ground into the slab since the last decade of the eighteenth century, and I see in them a magical connection with the disease itself. I further believe that the cup-marks are "unofficial"—by which I mean that they are the work of the superstitious populace of Lhasa, who have, in making them, practically obliterated the Chinese proclamation.

I understand that on the other side of this slab the proclamation is incised in the Tibetan character and that it is not defaced by pock-marks.

I saw this monument when I was in Lhasa in 1904 and made a rough sketch of it. I spoke of it to Dr. McGovern recently. As far as I am aware no one has as yet drawn attention to this obvious connection between the cup-marks and the small-pox.

N. V. L. RYBOT.

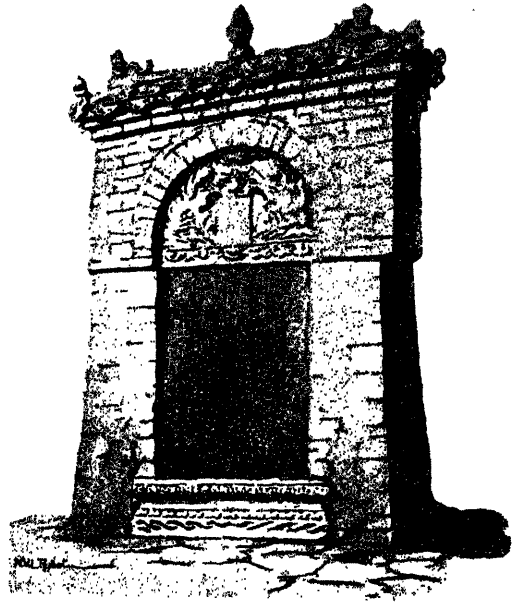


FIG. 1.—SMALL-POX EDICT PILLAR OR MONUMENT, LHASA.

Britain: Illegitimacy.

Brownlee.

On the Probability that the Distribution of Illegitimacy in the British Isles depends upon Survival of Custom from Definite Racial Invasions. *By John Brownlee, M.D., D.Sc.* **119**

I have been recently making some investigations into the conditions which are associated with the fall of the birth-rate. It was necessary in these circumstances to examine the illegitimate as well as the legitimate birth-rate. During these investigations some points have emerged which seem to have an anthropological bearing, and I propose to give a short note on the results obtained.

Much advance in the knowledge of race migration has been made in recent years by the study of the distribution and provenance of characteristic objects. Thus, the study of Anglo-Saxon jewellery by Mr. Leeds has thrown much light on the Anglo-Saxon settlements, while Mr. Peake's treatise on "The Bronze Age and the Celtic World," with its study of the distribution of bronze weapons, leads to many interesting suggestions regarding the dates of the Celtic movements.

The distribution of illegitimacy seems to offer another means of obtaining racial information. Customs often survive their introduction over extremely long periods and sex customs seem to have, in different parts of the world, great permanence. The validity of conclusions based on such distributions will depend, however, on the

evidence which can be adduced as to whether there has been much subsequent displacement of population.

It is not necessary to make any assumptions regarding movements of population. History records sufficiently well the nature of such movements. They are chiefly of two types: (1) where the whole or a large part of a population moves from one district to another: of this the Anglo-Saxon invasion of England may be taken as an example; (2) where a powerful and well-armed aristocracy conquers a people less well armed and disciplined: to this group the Norman Conquest of England belongs. A third method, however, exists where one race gradually infiltrates a neighbouring race. Such, to some extent, in France was the Frankish invasion, but it is better illustrated at the present moment by the migration of Italians into Provence. Of these three types, the second has probably happened most frequently in this country, twice historically—the Roman and Norman Conquests. The early invasion of the Celts with the leaf-shaped sword was probably a similar aristocratic conquest, and the invasion by the Belgæ immediately previous to the Christian era was probably between the first and the second. Judging from the sites of the Anglo-Saxon graves in the pre-Christian epoch, it would seem that the South of England was very much more affected than the North. The Angle graves north of the Humber are comparatively sparsely distributed in contrast to the distribution of Angle and Saxon graves in other parts of Southern England. This suggests the conclusion that in England north of the Humber and in Scotland, much less displacement of population has taken place than is commonly believed. In the hilly part of England which begins with that river, it must have been much more difficult to drive out a native population than in the flatter territories of the South. Using the hair and eye observations of Dr. Beddoe for the North of England, it would seem that even in the case of the Angle invasion the amount of new blood introduced cannot have exceeded 30 per cent., while it is well known that in the last invasion—the Norman Conquest—devastation rather than settlement was the fate of Northern England.

In investigating statistically the distribution of illegitimacy, a criterion of the amount is required. That chosen is the percentage of the number of illegitimate births to the total number of births. With this criterion it is found that in England south of the Humber there is little difference in the amount of illegitimacy from district to district. In Wales considerable variation occurs, but as I have little knowledge of this country these variations are not discussed. In the North of England, however, and in Scotland, the amount of illegitimacy varies greatly, and its distribution will now be examined.

In the North of England at the end of the Neolithic period and the beginning of the Bronze Age, as is well known, there was a very considerable immigration of a tall broad-headed race. With this invasion a form of burial commonly termed the "round barrow" is correlated. In the accompanying diagram (Fig. 2) the distribution of illegitimacy and round barrows is compared. The distribution of the round barrows has been taken from Lord Abercromby's "Study of the Bronze Age Pottery of Great Britain and Ireland." The distribution of illegitimacy is that for the registration districts taken from the statistics given by the Registrar-General for the years 1908-10. The boundaries of the registration districts are shown in both maps. The correspondence between the frequency of round barrows and the frequency of illegitimacy is, I think, too close to be due to chance. It would seem to indicate that this social custom was introduced at the beginning of the Bronze Age. If this correspondence is borne out—that is, if like correlations are found elsewhere—then it must be inferred that the amount of displacement of population in the North of England has never been very great, and that the Angles, though warlike, were never of sufficient number to do more than add a new racial element

to the inhabitants of the country. The amount of illegitimacy in the borders of Scotland will be referred to later. There are no head measurements, so far as I know, for the county of Yorkshire which would allow us to ascertain whether the region in which the round barrows stand has a broader-headed population than the rest of the country.

If only English statistics were available, it would be no more than a suggestion that racial invasions account for the custom. The evidence of Scotland is, however, of a somewhat different kind, and a more minute survey has been carried out. The numbers of legitimate and illegitimate births have been extracted for every parish in the 10 years 1901-10. This gives information of much greater detail than that given for the North of England. The range of variation is much greater, ranging from 1 to 37 per cent., the average for the whole country being 6.7 per cent. The high illegitimacy rate is specially associated with two districts: the counties adjoining Aberdeenshire and the southern districts of Scotland, especially Dumfries and Galloway. In Aberdeenshire, the area is associated with the broad head immigrations. The type of broad head found in the Aberdeenshire tombs is, however, of smaller stature and is much more closely allied to the Alpine race of Central Europe than to the invaders of Yorkshire and Southern England. At the present day the Aberdeenshire district contains a larger proportion of broad-headed persons than any other part of Scotland.

The amount of illegitimacy, taking the region as a whole, is from 12 to 15 per cent.—a rate which is comparable with that found in Central Europe at the present day.

With regard to the other district marked for its high illegitimate birth-rate—namely, Dumfries and Galloway—there is no broad-head association. The inhabitants are among the most narrow-headed in Scotland. When the map of the distributions of illegitimacy for this region was constructed, it seemed to recall some other type of distribution, and a comparison was made with other anthropological maps. The distribution was

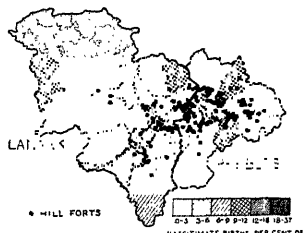


FIG. 1.

found to be very closely associated with that in the map of a type of hill fort in the South of Scotland given in Dr. Christison's memoir, "Early Fortifications in Scotland." These forts cannot be dated certainly, but Dr. Christison seems to suggest some period about the beginning of the Christian era \pm 400 years. They are not found to any extent in the area north of the Forth and Clyde. The association of the distribution of illegitimacy and of the hill forts is often very close. For instance, in the eastern side of Kintyre the amount of illegitimacy becomes much smaller as you advance northward and the hill forts disappear. Another good example is furnished by the islands of Islay and Jura. There are twenty hill forts in Islay

and in Jura none; the corresponding rates of illegitimacy are 15 per cent. and 5 per cent. respectively. Likewise, in Lanarkshire and Peebles there is a chain of hill forts marked to-day by a similar chain of illegitimacy (Fig. 1.). The North English border, as may be seen by the map, has also a high illegitimacy rate. This suggests

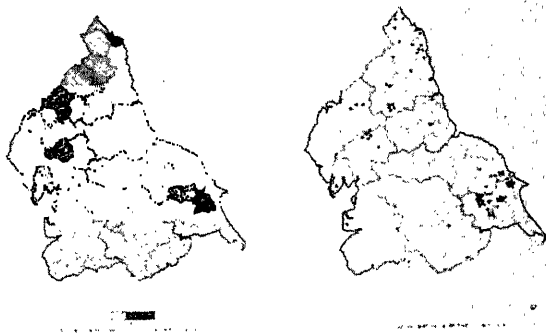


FIG. 2.

that the invaders of the South-west of Scotland also settled in the adjoining region of Northern England (Fig. 2), as happened in the later invasions of the Danes.

With regard to the rest of Scotland, the amount of illegitimacy is quite small. It is common to attribute the small amount of illegitimacy in Ireland to the Roman Catholic religion, but this religion is professed all through Central Europe and the illegitimacy rate is high. In many parts of Scotland the rate is as low, if not lower than, in Ireland. In the Norse settlements, Shetland and Orkney and the western islands, the rate is specially low, and through the Great Glen and most of Inverness-shire the same holds. In different parts of the north, however, there are small pockets where illegitimacy is high and as these are chiefly adjacent to the coast, they probably represent the results of settlements of small numbers of invaders.

JOHN BROWNLEE.

Anthropology, Physical: Disease.

Cunningham.

Some Factors in Racial Immunity and Susceptibility to Disease.

By Lt.-Col. J. Cunningham, B.A., M.D., I.M.S., Director, King Institute of Preventive Medicine, Madras. (Continued from MAN, 1926, 104.)

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Viewed from the biological standpoint, disease is merely the manifestation of the struggle which takes place between the invading organism and the body of the host. If the organism is immediately repelled, no evidence of the struggle may be forthcoming; but if it succeeds in maintaining itself for any time, poisons are secreted which disturb the harmony of the body, and a definite train of signs and symptoms is the result. As a rule, the defensive mechanism of the host in time overpowers the organism and recovery ensues; but, should the defence break down, death is the result. Under certain conditions a compromise between parasite and host may be effected, with the result that continuous infection without signs of disease may result, or continue after these have passed away. Such cases, known as "healthy" or "chronic" carriers, are of considerable importance as disseminators of disease.

Both the cellular and fluid elements of the blood take part in the defence of the animal body. The former by taking up and digesting the micro-organisms; the latter by means of substances known as antibodies, which have the power of killing, clumping and dissolving the organisms. These antibodies, bacteriocidins, agglutinins, lysins and the like, are capable of demonstration, but the extent to which any or all of these are present depends upon the type of the invading micro-organism and the severity of the infection.

It is not perhaps so generally recognised that the parasites are also capable of defending themselves to a considerable extent against the forces mobilised against them by the host. Organisms have probably become true parasites by a process of adaptation to their environment, and their survival in the past has depended upon their ability to withstand the opposition brought against them. Considerable variations may be induced in micro-organisms by exposing them to adverse conditions of growth either inside or outside the animal body. Such changes may be looked upon as a development of an immunity on the part of the organism to an unfavourable environment. Increase of virulence, capsule formation and acquired resistance to certain drugs may all be viewed in this light.

A more vivid impression of the methods of attack and defence adopted by both host and parasite may perhaps be gained from a concrete example which illustrates an extreme instance of organismal variation which we have lately investigated in Madras. Relapsing fever in India is characterised by an initial attack of fever followed by an interval, which in turn is followed by a relapse of the fever. The disease is caused by a spirochæte, the spirochæte *Carteri*, found in the blood during the febrile periods. At the end of each attack of fever the spirochætes may be

agglutinated and form clumps of varying sizes in the blood. We have lately found that the spirochaetes responsible for the relapse differ profoundly from those which are present during the first attack. This change is so complete that the formation of antibodies for each type is an entirely separate function. Either type of spirochaete may initiate the disease, in which case the spirochaete causing the relapse belongs to the opposite type. This complete "mutation" of the spirochaete has undoubtedly been adopted as a defence against the powerful antibody response on the part of the host.

The presence of antibodies, however, is not the only criterion of immunity. One attack of certain diseases, typhoid fever, for example, confers a lasting immunity, which remains long after demonstrable antibodies have disappeared from the blood. It must be assumed, therefore, that the body cells themselves take a prominent part in ensuring the efficacy of the defensive mechanism. Much of the natural immunity observed in different species and races of animals is undoubtedly due to this innate resistance on the part of the body cells, for natural antibodies, when found are only present to a limited extent.

Species and race resistance or susceptibility varies within fairly wide limits. In the case of certain species this may amount to an absolute immunity. A good example of this kind is the reaction of certain rodents to infection with anthrax. The guinea-pig and mouse are the most susceptible, rabbits less so, while rats are almost immune. If the extent of the susceptibility in each case is denoted by a series of + signs, the guinea-pig and mouse would be shown as + + +, rabbits as ++, and rats as a very small +. Immunity within races is much less marked and is one of degree only.

The *causes* which underlie these variations, and the way in which *they arise* and are *transmitted*, are of particular interest to our present purpose, for many of them are equally applicable to the racial differences said to be exhibited by man.

Certain instances of species immunity can be explained by the absence of conditions favourable to the development of the invading organism. Some organisms, for instance, require human protein for their growth and their attentions are thus solely confined to man. Simple cultural conditions of this nature, however, will not explain the majority of instances of species resistance, and this is even more the case where examples of racial and individual immunity are under consideration. Careful adaptation to its environment on the part of the invading organism is undoubtedly a factor in many cases, but, as far as the host is concerned, inheritance of the natural immunities and susceptibilities of the parent is the chief factor involved.

The predominating influence of *heredity* in perpetuating the *familial diseases** is fully recognised, but the exact way in which it operates in the case of the infective diseases is still an open question. Several possibilities exist.

Given sufficient time and a properly spaced series of epidemics of the necessary virulence, the law of the survival of the fittest may operate by the removal of the more susceptible members of the community, while those who are more immune escape. By this means a more resistant race will tend to develop. Evidence in support of this view is forthcoming in the fact that certain diseases undoubtedly pursue a milder course in races amongst which they have been long endemic, whereas they assume a much more severe and fatal form when introduced into a community for the first time. Measles and tuberculosis may be quoted as examples of this kind.†

* By which are meant diseases such as hæmophilia, colour blindness, and deaf-mutism and the like, which are handed down in families.

† In 1846 measles was introduced for the first time into the Faroe Islands and over 6,000 out of a population of 8,000 suffered from the disease. In the Solomon Islands in 1875, 40,000 out of a total population of 150,000 died from the effects of the disease.

Just as in other cases of *acquired characters*, no definite proof of the hereditary transmission of an acquired immunity is yet forthcoming. Specific antibodies in the blood of the mother can be transmitted to the offspring, but in such cases the germ plasma is not involved, for immunised males cannot transmit. Further, the immunity is only transitory in the offspring, and is lost in the second generation.

Evidence is accumulating, however, that definite susceptibilities and immunities can be transmitted along Mendelian lines and that these characteristics in certain cases are in the nature of antibodies residing in the blood. Von Dungern and the Hirschfelds have shown that races and individuals can be divided into groups according to the distribution of iso-agglutinins or bodies capable of agglutinating red blood corpuscles. These bodies may be transmitted by both parents and their distribution in the offspring conforms to Mendel's law. Rich has studied the hereditary behaviour of another substance normally found in the blood, known as complement, by crossing a race of guinea-pigs in which the character was found to be deficient, with normal guinea-pigs. He has shown that "the character known as complement is apparently controlled by selective mating in connection with hereditary variation" and that "entire conformity to Mendel's law was displayed in respect to the complement character." The complement deficient guinea-pigs were more susceptible to disease and the race ultimately died out from this cause.

Wright, Sewall and Lewis* have found marked differences in resistance to tuberculosis in a number of inbred families of guinea-pigs. "The high resistance of one family was transmitted by either sex in crosses with other inbred families." The experiments of Tyzzer and Little, Sly and others on the hereditary transmission of experimental cancer in mice, and the development of susceptible and resistant strains has a similar bearing on the present discussion.

Several instances of *natural immunity within races* of the lower animals are known to exist. The European sheep is more susceptible to anthrax than the Algerian sheep. The grey mouse is more resistant than the white mouse to streptococcal and pneumococcal infections, and, again, the field mouse is most susceptible (+ + + +) to glanders, the house mouse less so (+ +), and the white mouse almost immune (+).

Numerous examples of similar differences have been quoted in the case of *man*. Many of these, however, have been shown to be incorrect or to depend upon other racial factors. There still remain, however, quite a number of instances where it is claimed that a relative susceptibility exists on the part of certain races. Thus, the European is said to be more susceptible to yellow fever, diphtheria and intestinal diseases, while various dark skinned races are held to be more prone to tuberculosis and other respiratory diseases, elephantiasis and leprosy. The Mongolian is said to be immune to scarlet fever and the natives of endemic areas more or less so to malaria, while great stress has been laid on the comparative freedom from cancer of the less civilised races. Many of these examples are open to the objection that the comparisons have been made under conditions of environment adverse to the race said to be more susceptible. A much more accurate appreciation of certain of these claims at any rate can now be obtained by the use of tests, such as the Schick test for diphtheria, and others, which enable the true susceptibles or the infected in any community to be distinguished from those who are more immune, quite apart from any considerations of environment. Von Pirquet's tuberculin test, which depends upon the hypersensibility of the infected individual to tuberculin, has also been used for a similar purpose.

We may now briefly examine the grounds upon which certain of these claims are based.

* Quoted from "Heredity and Eugenics," by R. Ruggles Gates.

According to Rose-Carter, the increased resistance of the negro to YELLOW FEVER is due apparently less to the infection than to the toxin. He says: "Whether the negro contracts yellow fever less readily than whites may be a question, but that the case death-rate is less, and very much less, amongst them there is no question." He holds that this greater resistance is racial in that it occurs in negroes independently of a previous attack—an opinion which is contrary to that originally expressed by Sir Rupert Boyce, who believed that immunity in the native was acquired by repeated mild attacks in earlier life.

In the case of MALARIA, the immunity of the natives in endemic areas is said to be of this latter type. The greater the intensity of the infection, the earlier in life is the greater part of the community infected, with the result that a tolerance to the disease is acquired by the individual by the time adult life is reached. The mortality amongst the young is very high, and only those who have acquired this tolerance survive. Whether a more resistant race is eventually developed by this means is still an open question.

The greater liability of Europeans residing in the tropics to INTESTINAL DISEASES is frequently affirmed. That the white settlers suffer more commonly from bowel complaints than they do in Europe appears to be true, but whether the native is any less liable is open to serious doubt, at any rate in India. Cholera, fevers of the enteric group, and dysentery are the principal causes of morbidity as far as the alimentary canal is concerned. Cholera is no respecter of races, and attacks both Indian and European alike. The belief once held that the native does not suffer from typhoid fever has been finally dispelled by Greig and others. From extensive investigations into the prevalence of dysentery carried out in endemic areas, I have formed the opinion that nearly 25 per cent. of the native populations of these areas are infected.* "Carriers" have been frequently demonstrated amongst natives in the case of each of these diseases. It is more than probable, therefore, that the increased liability of the European residents arises from their contact with infected native servants, and not from a greater susceptibility on their part.

The extraordinary susceptibility of the negro and other less civilised races to TUBERCULOSIS has long been recognised. Certain anatomical peculiarities in the respiratory system of the negro have been frequently held responsible for this defect. Systematic investigations carried out in conjunction with the tuberculin test in various parts of the world have now shown that the principles which govern the prevalence of the disease in the urban and rural populations of European countries apply, broadly speaking, to races also. Countries which have been more or less civilised for a long time, and have thus had abundant opportunities for infection, show a high morbidity, but a comparatively low mortality, owing to the chronic nature of the disease. On the other hand, races which have only lately come in contact with civilisation show a low morbidity but a very high mortality, the disease under such conditions occurring in its most acute form. Bushnell gives as tropical examples of the first or tuberculised type of country, the tropical portions of the continent of Asia, the Philippines, Samoa, Hawaii, Reunion, Mauritius, Guadaloupe, Martinique and Cuba; and of the second or non-tuberculised type, tropical Africa and the greater number of the islands of the Pacific. The accompanying table, prepared by Lasnet and Trabaud, showing the relative

* Out of 1,100 routine samples of blood received at the King Institute, Madras, chiefly from native sources, 468 were positive to Widal's test for one or other of the typhoid group of organisms. Cultures of typhoid organisms were obtained from the blood in 15 out of 64 cases of fever in one of the Madras jails.

The "dysenteric index" (the proportion of latent infections in the supposedly normal population) in the Eastern Bengal jails was 19.8, and in two Madras jails was 30.9 and 27.6.

susceptibilities of the various races making up the troops forming the French army of occupation, illustrates this point very well:—

*Incidence of Tuberculosis per 1,000 Men during 12 Months—by Races.**

Français -	-	-	-	-	9.55
Marocains	-	-	-	-	11.73
Arabes -	-	-	-	-	13.33
Annamites	-	-	-	-	15.31
Malgaches	-	-	-	-	18.88
Senegalais	-	-	-	-	86.07

Similar conditions have been found to apply to the labour force in the South African mines. In India, Powell suggests that the almost total absence of the bovine type of the disease, which is responsible for a definite proportion of the tuberculosis of this country, may also be a factor in the lack of immunity displayed by the Indian native, especially those coming from rural areas.

It is now accepted as a fact that a relative immunity to tuberculosis does exist among races, but that it depends in degree upon the extent of previous contact with the disease. An innate immunity, such as that seen between species of animals, has not been found.

(To be continued.)

REVIEWS.

Gypsies : Linguistics.

Sampson.

The Dialect of the Gypsies of Wales, being the older form of British Romani preserved in the speech of the clan of Abram Wood. By John Sampson, Hon. D.Litt. Oxford, at the Clarendon Press, 1926. Pp. xxiii & 230 + 419. Price £4 4s.

The migration into North and Mid Wales of an 18th century English Gypsy named Abram Wood, and the subsequent isolation of his family and descendants from their fellow Gypsies over a lengthy period, may not seem to be important events in Gypsy history. But as a fact we owe to them the fortunate preservation in these islands—by a mere handful of people, it is true—of a Romani dialect that, despite certain phonetic and grammatical deficiencies, is better than most, and nearly as good as the best. And this dialect is destined to become famous among those who care for such things, since Dr. Sampson, who rediscovered it in 1894, when it was barely known to have existed and was believed to be extinct, has based on it the most comprehensive single book on Romani that has yet appeared in any language.

As the separate pagination implies, *The Dialect of the Gypsies of Wales* falls into two distinct parts. The first of these is a complete survey of Romani phonology, word-formation, inflection, and syntax, in which "Welsh Romani is dealt with not only as a form of speech worthy of study in itself, but as one of the large group

" of European Gypsy dialects which, " with the cognate Asiatic branches, " virtually constitutes an additional Indian " language, related to, though not derived " from, any of the seven sister vernaculars." No such work previously existed in English, so if Dr. Sampson had merely collected, sifted, and given a final orderliness to what is still acceptable in the researches of his predecessors, Pott, Miklosich, Ascoli and the rest, he would have performed a service of very real value. But besides doing this with a thoroughness and fairness and discrimination that are deserving of high praise, he has taken advantage of recently won knowledge of the Syrian, Armenian, and other Gypsy dialects, and of the Prākritis and Indian vernaculars, to advance, here a little, there a good deal, beyond the positions successfully occupied by the pioneers. Especially is this so in a long chapter on the history of Romani sounds, where, among many other things, Dr. Sampson is able to show that in Persia, perhaps about the 10th century, the original Gypsy immigrants divided into two distinct bands, one travelling north by way of Armenia and becoming the ancestors of the Armenian Boša and the European Gypsies generally, whilst the other, from whom descend the Nawar, Karači, and Helebis, journeyed southwards into Syria, whence some of them passed into Egypt and Asia Minor.

The second part of Dr. Sampson's great book is a dictionary of Welsh Romani

* After Lasnet and Trabaud. (From the summary of the original paper in the *Tropical Diseases Bulletin*).

whose fullness may be gauged from the fact that it occupies more than four hundred double-columned quarto pages. It has been the author's ambition ever since he discovered the dialect to record every root and compound, every variation in form and meaning, and every specialised usage; and certainly it would not pay any one to glean the field anew, even if it were possible at this stage. A glance at his pages reveals a number of words that are new to Romani itself, such as *bilanō*, gimlet; *nāi*, knee, and *velvel*, debt, which are Indian in origin; *butsa*, ball; *dārīka*, twelve, and *lutriā*, scullery, from Greek; *smaura*, raspberry, from Rumanian; *beligogos*, navel, from some Romance language; and *prextō*, fine, foppish, dainty, from German. Mention may also be made of words like *hufa*, cap; *glīda*, ember; *hērimentos*, wizard; *moila*, donkey, and *pevras*, to blink, which have their origin in dialect or archaic English; and of unique or rare compounds like *meurī*, necklace, and *moser*, to taste. Moreover, carefully considered etymologies are given throughout; and here, as in the grammatical sections, Dr. Sampson adds much that is new to the conclusions of his predecessors; whilst a classified index of the words cited as certain or probable etymons affords the highly interesting information that of these 517 are Indian, 54 Iranian (including Armenian), 1 Mongol, 81 Greek, 20 Rumanian, 56 Slavic, 17 German, 14 French, 151 English, and 36 Welsh.

In addition, Dr. Sampson, inspired in the first instance by Paspatis's great work on the dialect of the Turkish Gypsies, has illustrated both his grammar and his dictionary with countless examples of the actual speech of the Woods, drawing now on his collection of riddles and folk-tales (many of which have appeared in the *Journal of the Gypsy Lore Society*), and now on his store of records of conversations and chance remarks. The latter, as represented in the vocabulary more especially, furnish information on the rites and usages, taboos, superstitions, and folk-medicine of the Welsh Gypsies; and for that matter on many subjects besides. Indeed, it is no exaggeration to say that the examples of this class are scattered so freely, and are so well chosen, that they give almost all that is worth knowing about Welsh Gypsy life and lore.

T. W. THOMPSON.

Burma : Ethnography.

Brown.

Burma as I saw it—1889-1917.

By R. Grant Brown, I.C.S. London, 1926.

122

In this book we have a description of Burma and its people by M. Grant Brown, who served as a magistrate and district

officer in that fascinating country for twenty-seven years.

Mr. Grant Brown had the good fortune to be in charge of districts situated as far north as the Upper Chindwin, where he met head-hunting and human-sacrificing hill tribes, and as far south as Mergui, where he cruised in a sea-going launch amongst the beautiful islands of the Mergui Archipelago, hilly islands "wooded "down to their white beaches," and inhabited by roving sea-gypsies who call themselves Mawken, but are generally known as Salon.

The impressions of Burma left on the author's mind are so accurate that I cannot resist the temptation of summarising them. Of Arakan, a land of white beaches backed by dark mountains; of the far north, forest-clad mountains, swift rivers with hedgerows of wild-roses on their banks, patches of yellow rice-fields surrounded by dense forest, where "the "jungle-fowl came to feed in the cool "of the evening." The impression of Mandalay left on his memory is of hills topped with white pagodas, the King's Palace, "its tall pillars decorated with "glass-and-gold mosaic," in the background the blue mountains of the Shunshaks; of Central Burma, sandy tracks running between cactus hedges, fields of sessamum, cotton and tall millet, with here and there a cluster of palms marking a village. His picture of the delta is endless ricefields stretching away clear to the horizon and warm rain "falling "steadily day after day and week after "week." Of Rangoon, the Shwe Dagon-pagoda "blazing in the noonday heat, "or glowing red at sundown, or showing "through the morning mist on the "river, always a beautiful and inspiring "sight, typical of the country and a "fitting ornament to its gateway." Of Pagoda Point, the southernmost place in Burma, his memory is "a tent pitched "in a monastery garden among the "heavy-scented champac trees," clean white sands and rolling breakers.

The author has much of interest to say about the people, their customs, religious ceremonies and superstitions.

Not many foreigners are aware that Burmese children, "the most attractive "in the world," are born with a bluish patch of irregular shape on their lower sacral region, a patch that looks as if the child had been sitting on wet paint. The patch disappears after a year or so. It would be interesting to know whether this physical trait is universal.

The Burman is accused of being lazy, but the author maintains that "his is a

"reasoning indolence. It is accompanied by a careless good-nature and open-handedness, an easy tolerance and a desire to be courteous and considerate." He has no religious prejudices and he regards it as no concern of his if his neighbour (or any one else) chooses to "qualify for hell."

He is exceedingly kind to children and animals. "I remember," writes Mr. Grant Brown, "starting on a long ride one afternoon in the Pakokku district, when the thermometer registered over a hundred in the shade. After some miles I overtook a middle-aged Burman, who, with the sweat running from his face, was carrying a burden and leading a pony along the burning sandy road. The pony was saddled, and I asked the man why he was not riding. It was too hot, he explained, for the pony."

Burmese women are as independent as any in the world. They move about in public as freely as the men, and they commonly choose their own husbands.

Of the many quaint customs of the people, none could be prettier than the fire-festival, which is held yearly on a dark night in October, when every river and stream is "alive with many hundreds [or thousands] of lights." These lights are lamps or candles placed on tiny rafts and let loose in the stream. On the Irrawaddy river, where in places, it is possible to see for miles, a whole reach of the river may be ablaze with twinkling lights.

There is also a chapter on the stage. In Burma, play-going is a passion, and as plays last the whole night, the spectators take their bedding with them.

Every occasion, a birth, an entry into the priesthood, an ear-boring, a marriage, a pagoda-festival, an official visit, even a funeral, is made an excuse for a joyous outing.

It is to be hoped that at some future time the author will relate more of his experience, to which he has devoted a chapter that might well be longer.

The last chapter deals with politics. The author is not an admirer of the new "Reforms," by which we have endeavoured to give Burma an electoral system after the British model. His views, coming as they do from an officer of experience, merit consideration.

Mr. Grant Brown believes, and I agree with him, that with a suitable system of government, Burma has a great future before her, and that "we shall be indeed to blame if we do not set her on the road to a still greater happiness, and the Burmese people will have only

"themselves to thank if they do not climb from height to height of prosperity."

The photographs with which the book is copiously illustrated are excellent, and there is a useful map and index.

W. A. HERTZ.

Psychology.

Hazlett.

Ability. By Victoria Hazlett. **123**
London: Methuen, 1926. Pp. 147.
Price 6s. net.

This little book discusses certain aspects of ability, particularly in relation to intelligence tests. Confluence of experiences—i.e., the degree to which they influence each other in the solution of problems—is regarded as a general measure of intelligence, the degree of the confluence factor being determined by heredity. The writer emphasises the fact that intelligence does not develop, though its use increases with experience. While recognising that different degrees of inherited general ability exist, she is inclined to deny that special psychological abilities, even intrinsic, are inherited. The work shows an unfamiliarity with studies of mental inheritance, the pioneer work of Galton not even being mentioned; but its main intention is to consider the nature of ability and not its inheritance. Brief references are made to the careers of such geniuses as J. S. Mill, Corot, Hogarth, Newton and Cuvier, but they are too slight to constitute an analysis of mentality or of the possible inheritance of special abilities.

The second part of the book gives an account of the various intelligence tests which have been devised and applied to students of Bedford College. It was found easier to devise tests for science than for arts students. The results indicate a general correlation between success in the tests and in university work. Each of the science tests is regarded as measuring a special ability, and the results of the various tests appear to be on the whole unrelated to each other.

R. RUGGLES GATES.

Psycho-Analysis.

Freud.

Collected Papers, Vol. IV. By **124**
Sigm. Freud. Authorised translation
under the supervision of Joan Riviere.
London: The Hogarth Press and the
Institute of Psycho-Analysis. 1925. Pp.
508. Price 21s. net.

This completes the series of Professor Freud's collected papers (the first three volumes of which have already been noticed in MAN); an achievement of which

the publishers and translators have every reason to be proud, and for which all English-speaking students of psycho-analysis will assuredly be grateful.

The papers contained in this last volume fall into two groups. In the first of these are collected eight papers of a theoretical nature, in which Professor Freud endeavours to grapple with the very difficult problems of the theoretical interpretation of psycho-analytic discoveries. The remaining sixteen papers deal with applied psycho-analysis—the applications being made over a wide field, which includes literature, mythology, philology, fine art and sociology. Of all the collected papers, those in this section are probably of the greatest direct importance to the anthropologist, particularly interesting in this connection being the articles dealing with “The Antithetical Sense of Primal Words,” “The Taboo of Virginity,” “The Theme of the Three Caskets,” and “The Uncanny.” The book concludes with a useful chronological list of the contents of all four volumes of the series, to which have been added the titles and dates of Professor Freud’s publications in book form.

The translation has been made by various hands, but in accuracy and scholarship this volume quite maintains the high standard of its predecessors. It is needless to add that this is a work which deserves most careful study by all who are seriously interested in psycho-analysis and its applications. J. C. F.

Norway: Archæology. Shetelig.

Préhistoire de la Norvège. Par Haakon Shetelig. Oslo, 1926. Pp. 125 280 + . Price 7s. 6d. net.

Norway was certainly not the centre of the early prehistoric cultures of the Baltic area, and at sight of the title the reader is inclined to feel that the present volume will fall into the category of those purely national works which have at best only a local interest. But after reading a few pages it will soon be found that this is not so. Although, as the title implies, Norway occupies chief place, the book has a much larger significance. It is a really useful little work.

The first eighty-two pages are consecrated to the Stone Ages, an account being given of the Maglemosean and the Kitchen Midden cultures, the so-called Nøstvet industry (which is especially Norwegian), and the Arctic and true Neolithic cultures.

It is an excellent summary of recent knowledge, full of facts, but very readable.

Among other things one is glad to note that an account of the rock engravings of Scandinavian Art Group 1 is not omitted.

This little known series of rock drawings must probably be referred to the Arctic culture. They are of great interest and hitherto have been too often relegated to a note.

Only thirty-one pages are allotted to the Bronze Age, but here the limitations of the title come into play. The Norwegian Bronze industries are a poor affair, and not to be compared with those of Sweden and Denmark.

The Iron Age and the Roman Period are next discussed. The author has a very clear idea of the importance of the invasion of the North by the already ancient Mediterranean civilization. While the Roman arms were never actually seen in Norway, the influence of their culture was strongly felt, and a use of the expression “the Roman Period” is by no means a misnomer. It was also a time of cultural progress, as is evident to anybody who examines the finely decorated gold objects of this Age that have been found in the northern lands.

The volume continues with a short account of the Vikings and the ship burials, and concludes with a chapter on the decorative art in the Iron Age. There are a number of plates. M. C. B.

Psychology. Bernard.
Instinct: a Study in Social Psychology. By L. L. Bernard. London: George Allen & Unwin. 1924. Pp. 550. Price 15s. net. **126**

Professor Bernard belongs to that growing group of American psychologists who believe that the concept of instinct as used in modern psychology is a source of much confusion, misconception and error, and who endeavour to make its meaning more precise by confining its use to simple actions of the stimulus-response pattern carried out in virtue of a purely inherited mechanism. From this standpoint he reviews the treatment of instinct by many different writers and shows clearly enough that much of what they regard as instinctive should, according to his own conception, come under the category of (acquired) habit. Though carefully and conscientiously done, the value of this demonstration is surely questionable, since it is equally clear that most of these writers are working with a very different conception of instinct to that adopted by Professor Bernard and those of the school to which he belongs, according to whom there is little, if any, difference between instinct and reflex. The really important problem seems—to the present reviewer at least—to concern the relative justification and utility of the wider and narrower

views of instinct respectively, and it is greatly to be regretted that there is no serious attempt in this book to deal with the vindication of the wider concept of instinct recently attempted by McDougall (see especially *Journal of Abnormal Psych. and Social Psych.*, Vol. XVI, Nos. 5 and 6), who is generally regarded as the leading exponent of this wider view. McDougall considers that to reduce instinct to reflex threatens to emasculate psychology by depriving it of one of its most fruitful and important concepts—a concept that is of the greatest possible utility in interpreting human behaviour, both individual and social; the interpretation of human life in terms of simple quasi-mechanistic stimulus-response patterns must, he thinks, be at the cost of a serious

distortion and over-simplification of the facts and an abandonment of a most essential characteristic of the standpoint of psychology.

To the anthropologist the chief interest of the dispute will probably lie in its bearing on the problem concerning the relative importance of heredity and environment in determining human culture and behaviour. The present volume, in spite of what seems to us the serious deficiency to which we have drawn attention, is, so far as it goes, a clear and forcible statement of the anti-instinctivist position—a statement which will, perhaps, be all the more interesting to British readers since the standpoint it defends has found comparatively few supporters on this side of the Atlantic.

J. C. F.

CORRESPONDENCE.

Malta: Archæology. Caton-Thompson.
"Excavations in Malta."

To the Editor of MAN.

SIR,—In regard to the review (MAN, 1926, 107) of "Excavations in Malta" by M. A. Murray and G. Caton-Thompson, Part II, may I be allowed to amend your reviewer's statement that I am "inclined to support" Dr. Ashby's tentative surmise, later more positively pressed by Sir Arthur Keith, that the bone breccia deposit in Ghar Dalam is a kitchen-midden of Palæolithic age.

My words are " . . . I am, in the "almost complete absence of evidence to support it, unable to accept a view which "has been expressed . . ." etc. I then give my reasons (pages 9-10).

Yours faithfully,

GERTRUDE CATON-THOMPSON.

India: Archæology. Hocart.
"Phallic Offerings to Hathor."

To the Editor of MAN.

SIR,—Mr. G. D. Hornblower, in his article on "Phallic Offerings to Hathor," (MAN, 1926, 52) reports that the incident of a donkey and a woman *in coitu*, which occurs in Apuleius, is illustrated several centuries earlier on Egyptian glazed faience.

The same subject is not uncommon in India. I have seen several examples, notably in Bijapur. It occurs on grants recorded on stone. The important point to note is that it is not a mere display of obscenity, but illustrates a curse or vow, just like the sun, and moon, and other

objects illustrated on these grants. I was told that the text prayed that he who violated the grant might be called the son of an ass. Mr. S. Paranavitane, my Epigraphical Assistant, refers me to "Epigraphia Zeylanica," vol. IX, plate facing page 179, for an illustration (A.D. 1065). There is, however, no allusion in the text. On page 164 we are told that the Kuruspāl inscription has a representation of an ass associating with a pig, the imprecation being explained in the text thus, *Jo (yo) anyathā karoti tasya pitā gardabhā sūkari mātā* (he who acts otherwise has for his father an ass and for his mother a pig).

One is tempted to bring these facts into relation with an incident at the great horse sacrifice in the "Satapatha Brahmana," XIII, 5. 2. 2., where the queen lies with the dead horse and is supposed to be impregnated by it (Eggeling's translation omits the more pointed details. The text is very explicit). Further, see Nissanka Malla's inscription at Polonnaruwa, published by Mr. D. M. de Z. Wickremasinghe, in "Epigraphia Zeylanica," vol. II, p. 164, where the royal caste is said to be to other castes as the horse is to the donkey.

Putting all these hints together we may have some notion where to look for the origin of the idea. The clue is worth following up; for if the origin is ritual we should have a definite case of ritual origin of an idea which every one would at first sight confidently declare to be the immediate outcome of an obscene mind. It would be another case of psychology going astray when not guided by history.

A. M. HOCART.



FIG. 1.



FIG. 2.



FIG. 3.



FIG. 4.

A SURGICAL OPERATION AS PERFORMED BY THE BOONARRA TRIBE OF
NORTHERN AUSTRALIA.

ORIGINAL ARTICLES.

With Plate M.

Australia: Surgery, Linguistics.

Terry.

A Surgical Operation as performed by the Boonarra tribe of Northern Australia, and a short vocabulary of the languages of some North Australian Tribes. By Michael Terry. (With Plate M.) 129

The members of the Boonarra tribe who inhabit the region surrounding Billiluna station at the terminus of Sturt Creek still observe the practice of opening the artery in the fork of the left elbow. This they do to obtain a supply of blood for use as gum, wherewith to adhere tufts of flax to the bodies of dancers in the corroboree. So far as I could learn, it is not the practice for each man to open his artery, but one who will not take part in the dance is selected for the operation. He is given a length of hair string about three feet long, which he ties as a ligature round the bicep, encircling the arm about three times. He holds one end in his mouth while he twines the ligature round, using his right hand; the loose ends are tucked under. Then he proceeds to make an incision with a small piece of stone where the artery is only just beneath the skin. He does not scrape away the flesh, but jabs in with the stone till the artery is punctured. When a flow commences he gives additional punctures till the blood issues freely, keeping his left hand tightly clenched meanwhile. It may have been a coincidence, but it was noticeable that the man who opened his artery had hair that stood up very straight, whereas the other men had theirs trained straight back, either by design or by natural inclination. Having allowed the blood to issue till a small pool had collected upon a piece of bark at his feet, he sealed the wound by pressing the stone used for the incision over the cut.

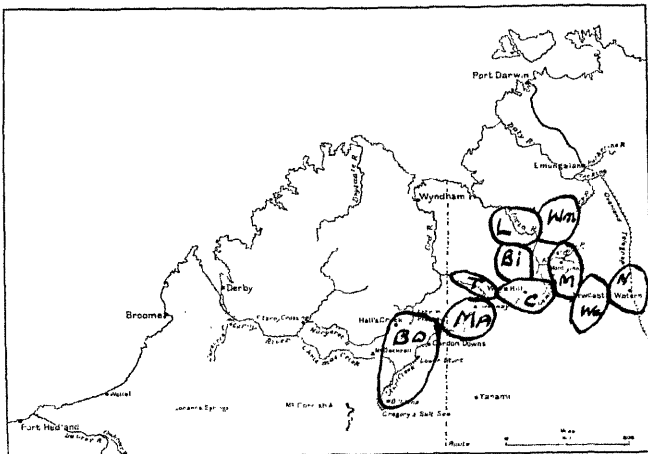


FIG. 1.—DISTRIBUTION OF TRIBES.

This he held in place by tying the hair string, removed from its duty as ligature when the operation was being terminated, over it in the same manner as when it was being used over the bicep.

There does not appear to be any religious ceremony attached to this operation, nor to the blood obtained, for it was noticeable that dogs were allowed to lap up what was not required by the dancers.

It always appeals to me to seek out the differences between the coastal aboriginals and those living remote from the sea. Not only in customs, but also in languages do we find this separation into two main groups. With this in mind I selected, during my recent expedition through Northern Australia, a number of the most common words and have here tabulated them, not to exhibit once again the enormous dissimilarity in tribal languages, but to see if there may not be some governing law which determines this changing. The spellings are purely the closest phonetical copies after hearing the words repeated several times and after they have been used with success to other blacks in conversation. Care has been taken to differentiate between the sounds "n" and "kn," for it is noticeable

that "k" replaces the pure "n" the further one moves inland—*vide* "woman" and "water." In fact, to me, there appears to be a general tendency of all sounds to harden as the coast is left, as in "moon."

Tribes.	Location.	Tribes.	Location.
Nanaroola (N)	Newcastle Waters.	Warramulla (Wa)	S.E. of Montijinnie.
Mootburra (M)	Montijinnie.	Tookurra (T)	Around Stirling river.
Wadderman (Wn)	Dalamere.	Manoo (Mo)	Inverway.
Loonga (L)	Wyckham.	Boonarra	Flora Valley.
Coorinji (C)	Wave Hill station.	Boonarra (Bo)	Halls Creek.
Bilinurra (Bi)	nr. Mt. Sandford, nr. ditto.	Boonarra	Gregory's Sea.
Mootburra (Ma)	East of Wave Hill.	Tchagilin (language)	Gordon Downs.

Interpreted by Nimarra (f). Given by Dilirri (m).			Tchagilin language. Louie and Yerry (m). Rowan.		
		Nara-ragoo (m)	Tadnarri (m).		
Tribes	Mootburra	Cooringi	Manoo	Boonarra	Boonarra.
Water	moko	moko	knawa	narpa	knarpa
Wood	warloo	korndi	jowie	{ korndi boomoo }	{ — warloo }
Head	warloo	warlloo	warloo	lunga	{ warloo lunga. }
Fire	warloo	warloo	—	—	warloo
Flour	mungaree	mungharee	mungharee	bulba	tarmba
Tobacco	wallairah	kangillah	woonju	tchangnu	tanjunga
Beef	narareena	knarid	knaril	buoya	knandi.
Woman	giri	janga	janga	naringa	knaringa
Boy	nalka	meeut	meeut	marben	knanbai.
Turkey	kwokabudee	kwokabudee	{ jamut wiloojarji }	janjura	bengirijaddu.
Wheeled vehicle	weel-barra	weel-barra	weel-barra	weel-barra	—
Bamboo	didjiri du	kinjuno	bamboo	bamboo	taragi.
Axe	warra-warra	urlga	wilga	wilga	mayuga.
Stick	kendi	korndi	{ poonoo korndi }	boonboo	manna.
White man	cudibah	yadoo	cudibah	ooljung	cardiah.
Sun	wangoo	wangoo	kangari	{ warloo kangari }	{ brangoo. waigin. }
Tree	—	korndi	korndi	boonboo	y-warradadu.
Emu	knaril	ybarradoo	ybarradoo	waniarrou	tadji.
Kangaroo	wangoorra	giah	{ geeah tadji }	geeah	jagan.
Moon	budda-narra	jakillie	jagallin	jagallin	—
Whirlwind	—	wigaroo	woomat	{ mailloo u-anna }	—

Additional names from Billiluna —

Sleep	mucken.	Nose	jig-je.	Tucker	mee.
North	kyra-ra.	Eye	mil.	Boree	wiemangaroo.
South	cooleera.	Ear	mungilgil (ghard).	Possum	jang-ad-na.
East	cuckera.	Stomach	munda.	Mountain devil	merdi-jibil.
West	culera or karla.	Chest	dongal-dongal.	Spinifex	juga.
One	khyan.	Mouth	lira.		
Two	cudjarra.	Fingers	knarhe.		
Three	cooering.	Arm	kilbilee.		
Five	pongeroo.	Hair	milgel.		
Big	yumpi-yumpi.	Black	culeur.		

Also collected at Billiluna:—

Sit down	loo-loo.
Stand up	burro.
Go straight in the lead	cumbera-yando.
Finish (referring to water)	munduj -in desert i.e. nagida.
Full up (referring to water)	mundudji -in desert i.e. krudangaba.

MICHAEL TERRY.

Persia: Religion.

Ivanow.

Muhammadan Child-killing Demons. By W. Ivanow.**130**

Persia for many centuries exercised a great cultural influence on neighbouring countries; the conservative spirit of her inhabitants has saved from oblivion many customs and beliefs that descend from remote antiquity. These are often disguised in a Muhammadan garb, but they can be identified with comparative ease.* It is, therefore, amazing to find what little anthropological research is done in the country among the various races inhabiting Iran, or in the extensive material offered in Persian literature. Only of late have several Oriental students done work in this direction,† but the literary materials are scattered in a great number of manuscripts belonging to different libraries, and their examination cannot be undertaken by a few workers. Besides, some manuscripts existing only in the unique copies are not easily accessible.

For this reason it would seem right that every Oriental scholar who comes across interesting material in any rare work should make a note of it for his colleagues—anthropologists—who thus would be enabled to make proper use of the information so obtained.‡ In this article I give a few extracts from a rare Persian mediæval medical treatise written in India, probably at Delhi, in 778 A.H., or 1376 A.D.,§ which reveals some peculiar beliefs concerning the supernatural malevolent beings which cause death or harm to young children.

Although Muhammadan medical authors often give some special advice or recommend various amulets for protecting children, they usually take knowledge of these demons for granted, and almost never give a proper description.|| The value of the work under consideration is entirely due to the comparatively detailed information which the author gives on this point.

This work is called *Ráhatu'l-insán* (or "The Comfort of Man"), and deals not only with purely medical matters, but also with various prescriptions of a magical nature. The author, 'Abdu'l-Qawí ibn Shihábi'd-dín, surnamed *Diyá* (pronounced *Ziyá*), dedicates it to the sultan of Delhi, Firuz III., who reigned from 752 to 790 A.H. (or 1351–1388 A.D.). It is singular that the learned doctor, *Ziyá*, not only knows thoroughly Muhammadan occult literature, but also shows a knowledge of Hindu works of this kind, and very often uses incantations that apparently are in the Sanskrit language.¶ These, however, are difficult to understand as they have been greatly mutilated through Arabic transliteration, and also by the scribes who copied the manuscript.**

The author, apparently in agreement with Hindu beliefs, divides all evil beings into five classes (f. 44v): those who live in decaying refuse, in carrion; those "who

* The folklore of the Muhammadan nations is often treated as having a comparatively modern origin. It seems, perhaps, more correct to attribute considerable antiquity to it. Islam as a religion was continually at war with local popular beliefs, and only those superstitions survived (in a Muhammadan garb) which were strong enough to withstand all adverse circumstances, especially in Persia, as deeply rooted in the psychology of the people.

† The name of Professor A. Christensen can be mentioned in this connection.

‡ I have tried to publish a similar note. See my article, "A Witch Case in Mediæval India," *Journal of the Asiatic Society of Bengal*, vol. xix. (1923), pp. 43–50.

§ It is briefly described in my "Concise Descriptive Catalogue of the Persian MSS. in the Collection of the Asiatic Society of Bengal." Calcutta, 1924, No. 1535 (on p. 716). Apparently no other copies of this work are known in other libraries.

|| A brief account of demonological ideas is often given in the Muhammadan encyclopædias. Unfortunately, they pay no attention to these "popular" species of evil spirits, and concentrate on *jínns*, etc.

¶ The perusal of Hindu medical literature by Muhammadan authors became a general practice much later on, in the sixteenth and seventeenth centuries.

** The present copy is modern, dating from the beginning of the eighteenth century, and thus is separated from its original by four hundred years.

"become spirits after the bodies of idolaters or Jews are burnt, and who, joining "the wind, whirl about the desert"; changelings;* and, finally, those classes of *diws* and *peris* who seduce humans and turn them towards sin.

It is apparently to the last-mentioned class that the vampires attacking the child belong. The author distinguishes two species: Ummu's-sibyan and "shape-shifters," or were-hyenas, *kaftár*.

Ummu's-sibyan, "the mother of children," as she is called in Arabic, is probably an ancient conception of the spirit causing the premature death of new-born babes. It is apparently the same as the Jewish Lilith, or lamia, of the ancient world, or the strigle of European nations; Ziya identifies her with the Indian Mirgi (f. 43).† In Persia nowadays she is known only to the educated under this, her "scientific," Arabic designation. The peasants use different "taboo" expressions. In the districts of Qain and Birjand (Eastern Persia) she is called *Modare-shaw* (*sinä likitaw*)—i.e., "Night mother (with long hanging breasts)." In Fars she is politely called *Al* ("family"), or *Bakhtak* (diminutive from *bakht* = good luck), with a strange epithet *uftáda-rúsh*.‡ In Northern Khorasan, in the Sabzawar district, she is styled *Khala-khosh* (*khála-khášh*)—i.e., Mrs. (literally, aunty)§ "mother-in-law." She is a miserable-looking old woman who comes at night to strangle new-born babes; she is immortal "like a cypress, and like the raven (who has access to the water of life)." A woman in childbed must not be left alone in the room, for this old woman will then come in, kill the mother and strangle the baby. To frighten her, some kind of weapon is hung on the wall.

These are the details given by Ziya (f. 63v): "Ummu's-sibyan is the mother "of devils (*diws*). She has at her breasts seventy baby-diws. When a woman "gives birth, she also disburdens herself, and, if not watched, may remove the "(human) child and replace it by one of her own."||

Thus she is the mother of changelings (f. 44v): "The *khábith* (a sort of evil "spirit) is he who says: I am a Muslim, I am a Sayyid, or I am so-and-so, whatever "the case may be. But (in fact) he is a changeling, a *diw* who is born at the same "time as the human child. When a Muhammadan woman gives birth to a child, "Ummu's-sibyan, the mother of *diws*, also gives birth to one who is similar, and "also gives him the same name."

There are different methods of getting rid of this demon. One of them is through the smell of burnt hair from the neck of a black cat (f. 63v): ¶ "If some one, or one's "child is attacked by Ummu's-sibyan, the remedy is as follows: one must take "a black cat, pick up a hair from its neck and burn it in the house; Ummu's-sibyan "will flee immediately."

A tablet with a special incantation on the neck of the child will keep her off (f. 43). Or special ointment can be prepared (f. 88), and applied to the baby; it is only necessary to be careful that at the time of applying it no hole of a mouse or a snake is near by.

* This word is here used in the same sense as in Hastings' Encyclopædia, vol. iii., p. 358.

† She is a female demon personifying tetanus and other child diseases.

‡ Perhaps this means "with emaciated, fleshless face"?

§ Literally "maternal aunt," but generally used when addressing a woman, even should she be no relation.

|| Not rarely she is also described as a "childless woman," one whose children die as soon as they are born.

¶ Black cats and dogs are credited in Persia, as elsewhere, with having a special connection with the supernatural world. In a treatise dating probably from the earlier half of the sixteenth century (see my "Descriptive Catalogue of the Persian MSS. in the Curzon Collection, Asiatic Society of Bengal," Calcutta, 1926, No. 652, on p. 440), the belief that black dogs are in fact *jinn*s is ascribed to the authority of Muhammad himself.

In a short metrical incantation which I heard in 1920 in the district of Sabzawar, North-Eastern Persia, there are strange allusions to some other methods, apparently the sacrifice of a dog or cat, perhaps for propitiating the demon :—

Khalakhoshum khalakhoshum
sake zardre bekushi beroye ow-oshe khoshum
khalakhoshum khalakhoshum
kerde dele mu nokhoshum
gurbaye zarde jigh kuni
beroye selot-kushe khoshum
dure dure bobure
poshune guresh bobure
mebodo ki war garde khoshum.

“ My mother-in-law, my mother-in-law !
 Kill ye a yellow dog* for the feast of my mother-in-law !
 My mother-in-law, my mother-in-law !
 She made my heart sick.
 Call a yellow cat,
 For a sacrifice† to the mother-in-law.
 Let her take it far, far away,
 to the bottom of the grave,
 may mother-in-law not return ! ”

The second demoniacal being to attack the child is the *kaftar*. This name means a hyena, a rare animal in Persia. The conception resembles the Arab *ghul*, or Kurdish (in Northern Khorasan) *murdor-muy*. Apparently these imply a witch, or a strange animal-like looking being, having not much connection with the usual lycanthropic ideas. References to *kaftar* are very rare in Persian literature, it is difficult to find whether or not this belief is popular in Persia nowadays—I have never heard it. But it may survive under a different name, and therefore the following details, given by Ziya, may be useful for identification :—

(f. 41v). “ Chapter xxxiv., on remedies of the diseases of young children.”

—“ If in a hamlet, in a village, or in a town there a *kaftar* exists, one must recite the following incantation, shearing some hair off his own left hip. The head of the *kaftar* should be cut off by this. This act must be done on a Sunday, during the first hour. The formula is this. . . ‡

—“ On detecting a *kaftar*. If someone is suspected of being a *kaftar*, four copper coins, *daniks*, must be taken, the following incantation read, and they must be thrown on the ground. The suspected one must be asked to take these coins up. If a *kaftar*, he will not be able to lift them, and it will be known that he is a *kaftar*. Otherwise he will be able to lift them. The formula is this. . . ”

—“ If someone is suspected of being a *kaftar*, one must recite the following incantation over his own finger, blow on it, and then draw a line three times on the road by which the suspect is coming. If he is really a *kaftar* he will never be able to pass until the line is erased.”

—“ And if one would pass the road of a suspected *kaftar*, and draw a line placing at the ends of it leaves of a species of willow, called *bīd-i-anjīr*, and then, turning

* In Persia nowadays the dog is regarded as the most unclean animal. The grey or yellow colour is almost universal amongst the canine population of the country; black animals are very rare.

† The expression which is used here is not the same as that which is applied to the ordinary sacrifice prescribed by Muhammadanism.

‡ These formulæ are either meaningless words, being partly misspelt Sanskrit expressions, as mentioned above, or Arabic words or short sentences, which have no coherence. All these are here omitted for the economy of space.

“ to the line, will recite the incantation, and blow, then, if the suspect really is a *kaftar*, he will never be able to cross the line. The formula is this: In the name of the Merciful, the Compassionate! O the deed of those who act! By the truth of ‘*we worship Thee and we seek help from Thee!*’ ”*

—“ If one would fill up an earthenware vessel with water, dropping a little iron on its bottom, and recite once the verse II, 256 of the Koran,† and blow afterwards and then ask the suspect to lift it, the latter will never be able to, should he be a *kaftar*. ”

—“ If one suspects somebody to be a *kaftar*, he should take early on a Sunday a steel needle (f. 42v), and hiding himself (wait till that man comes up). As the latter passes, he (the performer of the magical act) must follow him, and when the suspected *kaftar* stops, and his shadow is on the ground, he must pin his shadow with that needle. If that man really is a *kaftar*, he will never be able to move from that place; if not, he will pass freely. ”

—“ If a child has been attacked by a *kaftar*, one must draw a circle on the ground with a knife, and spit in the middle. Then one must strike the knife, with firm decision,‡ on the spot where he spat, at the same time reciting the following incantation. Then the knife will go into the navel of the *kaftar*, and he will scream and leave the child. The formula is this. . . .§

—“ To repel the *kaftar*. One must write the following incantation, and keep the charm; then even if all the *kaftars* in the world should come together they would be unable to do any harm to him. The formula is this. . . . ”

—“ To repel the *kaftar*. One must recite the following incantation, seven times, over a piece of thread, make seven knots on it, and then tie it round the child's neck (f. 43); the *kaftar* will pass near him, but will be unable to notice him. This is the incantation: *salatah tahu*. ”||

—“ To brand the *kaftar*. If one should want to brand a *kaftar*, he must take cow dung and . . . ¶ and heat an iron brand. When he has applied the brand three times to the cow dung, the child (affected by the *kaftar*) will recover, and the mark will appear branded on the breast and the face of the *kaftar*. The formula is this. . . . ”

(f. 72v).—“ To repel the *kaftar*. One must draw the following charm figure on a piece of paper, or wooden board, and fix it firmly over the entrance to the house. The *kaftar* will never pass that side of the street, and will never return. And if one draws the same figure on a tablet of gold, silver, iron, or copper, and hangs it on the neck of a child, the *kaftar* will never injure him, nor look towards the side where the baby is, nor will he even remain in the part of the town where such a child lives. . . . ”**

This note may be concluded by a passage giving a useful recipe for guarding a child against other evil spirits.

* Koran, I., 4.

† This verse, called *ayatu'l-kursi*, is very often used in magical prayers, together with a few others.

‡ Literally: “in firm condition.”

§ A long Arabic prayer follows.

|| In Southern Russia a thread of red wool, on which seven knots are tied, is placed round the neck of one who suffers from toothache, and tied with the eighth knot. The pain immediately ceases.

¶ Illegible.

** In Persia, especially in the province of Fars, it is customary, should anyone be ill in the house, to draw something like a figure of a man on a sun-dried brick, on which a little salt, an onion, some charcoal, a nail, and a few copper coins are placed. This is turned round over the head of the patient several times, and then brought into the street and laid near the wall. The patient will recover in due course.

(f. 88).—"To safeguard a child from *diws*, *peris*, *kaftar*, and epidemics (literally : adverse winds). When a babe comes out of the womb (of its mother), he must be smeared seven times with butter. One must get a feather from the right wing of a cock, soak it in the butter, and then burn it, waving a little of that smoke towards the child. The baby then will be immune from the attacks of *diws*, *peris*, *kaftars*, and Ummu's-sibyan."

W. IVANOW.

Anthropology, Physical Disease.

Cunningham.

Some Factors in Racial Immunity and Susceptibility to Disease.

131

By Lt.-Col. J. Cunningham, B.A., M.D., I.M.S., Director, King Institute of Preventive Medicine, Madras. (Continued from MAN, 1926, 120.)

The information so far available with regard to DIPHTHERIA points in a similar direction.

Observations on the percentages of the susceptibles and immunes obtained by means of the Schick test in various countries show considerable variations.* The proportion of each type occurring in any community once more appears to depend upon the extent of previous contact with the disease.

The reputed immunity of the Mongolian to SCARLET FEVER does not appear to exist in fact.

The available statistics, although somewhat contradictory, appear to show that the disease is more prevalent and virulent in North than in South China.† It is probable, therefore, that other than racial factors are likely to be responsible for the divergence which has been claimed between the Mongolian and other races in the past. The application of the newly-introduced Dick test along lines

* The percentage of positive reactions (susceptibles) to the Schick test in children in England and America varies between 60 per cent. and 70 per cent. in the better-class areas and 15 per cent. and 20 per cent. in the crowded sections (Park, 1922), the difference being explained by the greater opportunities on the part of the latter of coming in contact with infection. In adults the percentage lies between 15 and 18.

The proportions of positive reactions reported in various countries is as follows :—

Positive reactions.		
Countries.	Per cent.	Authority.
Philippines - - -	8.5	Gomez, Navarro, and Rapanau (1922).
Algiers (primary schools) -	29	Sergent, Béguet, Parrot, Lemont (1922).
Constantine - - -	42	Ciavaldini (1922).

(The percentage of European positive reactions was twice that obtained in the natives).

Thursday Island - - - 96.8 Metcalfe (1924).

(The population consisted of both whites and natives. The disease has occurred here very rarely in the past).

So far as adults are concerned the negro in America gives the same percentage of positive results as does the white. (Wright, 1917).

† Yang Ting Kuang and Shih (1924) state that "Scarlatina is practically absent or is very mild in South China, not unduly severe in Shanghai and Central Provinces, and severe in the "North." They quote the following statistics :—

Lennox health statistics for Chinese missionary families :—

	Cases in children.	Deaths Per cent.
Total - - - - -	193	—
North China - - -	104	12.5
Central China - - -	85	2.3
South China - - -	4	0

At the Pekin Government infectious diseases hospital out of 638 cases (1915-23) 20.8 per cent. died.

Stanley reports (1917) for Shanghai—1,500 Chinese deaths occurred in 1902. The disease was of a virulent type both among Chinese and Europeans. Lafont (1923), on the other hand, reports an epidemic in Yunnan (South China) where 50,000 out of 200,000, chiefly children were carried off.

similar to the Schick test in diphtheria will undoubtedly place the whole question on a much surer footing.

Little is known with regard to the supposed racial susceptibility to LEPROSY and ELEPHANTIASIS on the part of the native. There is no scientific evidence in support of the view commonly held by the lay public that an admixture of dark blood increases the liability to these diseases. Rogers and Muir deny that the white is less susceptible to leprosy than the native of India. The higher standard of sanitation adopted by the white races is in itself sufficient to account for much of the disparity which occurs in the incidence of the diseases in the different races.

Our views with regard to the incidence of CANCER amongst the lower races has lately undergone a profound change. The native can no longer be considered naturally free from this terrible scourge. Comparable statistics for this disease are very difficult, if not impossible, to obtain; but the general opinion now held by those who have investigated this question in the tropics is that there is little, if any, difference between the incidence of malignant disease amongst the European and non-European races if similar age groups are compared. The types of cancer and the organs involved differ widely, but similar predisposing causes are undoubtedly at work in each case. Thus, the tar and soot cancers of European countries are represented in the East by the "Kangri" cancer in Kashmir; the beetle-nut chowers cancer in the mouth in Southern India and other countries and the epithelioma of the skin over the hip in China due to the habit of sleeping on the hot k'ang or brick-oven bed. Cancers of the alimentary tract* are rare in Eastern races, but their place is taken by a primary carcinoma of the liver which appears to be one of the most common causes of malignant disease in the East. This type of cancer frequently follows a cirrhosis of the liver. Cirrhosis of the liver is very common in young native adults and is variously stated to be due to malaria, highly-spiced foods, or to the absorption of poisons from a gut previously damaged by dysentery. The predisposing factor common to all types of cancer, namely, chronic irritation, is therefore to be found in this case also, although the particular organ affected differs in the native and European.

Snijders and Straub† show that the types of cancer may also vary amongst the Eastern races themselves, as the following figures show:—

	Stomach.	Œsoph.	Intest.	Liver.	Ovary.	Uterus.	Mammæ.	Skin.	Penis.
	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.
<i>Javanese.</i> Male : female = 2 : 1	1	0	2	55	3	16	2	7	5
<i>Chinese.</i> Male : female = 10 : 1	19	4	3	28	1.2	0	2.8	9.3	10.1

* Cancer statistics in white races. (From Ewing, "Neoplastic Diseases," 1922).

Stomach.

Welch (1885), 21.4 per cent.

Gert (Vienna), 10 per cent.

Martin (1908), 33 per cent.

Haberlein (Switzerland), 41.5 per cent.

Reiche (Hamburg), 35.5 per cent.

Virchow (Wurzberg), 34.9 per cent.

Liver (primary).

Orth, 0.28 per cent.

Goldzieher and Bokay, 1.3 per cent.

Orth, Hansemann and Rindfleisch, 0.5 per cent.

Uterus.

Welch, 29.5 per cent.

Orth, 30 per cent.

Williams, 22.5 per cent.

† "The relative frequency of carcinoma in different organs in the Javanese and Chinese." Snijders and Straub. (Taken from a summary of the original paper in the *Tropical Diseases Bulletin*).

In the Javanese, primary cancer of the liver and uterus are more common than among the Chinese. Cancer of the stomach affects the Chinese to a certain extent, but hardly ever affects the Javanese.

So far, racial differences of this type only can be demonstrated. The true significance of such variations must await the discovery of the immediate cause of malignant growths in general.

I think that we may safely conclude that no innate immunities exist between the different races of men comparable to those found between different *species* of animals. A type of *racial* immunity does occur, however, due, as far as is known at present, to previous contact with the diseases in question. Such an immunity appears to be akin to the few examples of varying racial resistance known to exist amongst the lower animals, but in the case of man it is not so well marked.

The manner in which a racial immunity is transmitted is still uncertain, but evidence on this point is steadily accumulating. It is probable, however, that the chief factor in the evolution of a more resistant race is the gradual elimination of the susceptibles.

I have endeavoured to lay stress on the close relationship which exists between social environment and disease. It will be noted that many of our medical problems require the skilled assistance of the anthropologist for their solution.

J. CUNNINGHAM.

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Stone.

Stonehenge—Concerning the Sarsens. By E. Herbert Stone, F.S.A. **132**

What was the source of supply of the great sarsen stones now forming the greater part of the structure of Stonehenge?

In the work on Stonehenge† by the present writer the subject of the origin and nature of the stones is treated somewhat fully, and opinions are quoted of geologists

* Consulted in abstracts published in the *Tropical Diseases Bulletin*.

† "The Stones of Stonehenge," by E. Herbert Stone, F.S.A. London: Robert Scott.

and other authorities who have dealt with the matter. In this article, therefore, to avoid repetition, the conclusions arrived at will merely be stated briefly, giving references to the pages of the author's work on Stonehenge where the data for these conclusions are set forth in detail.

In the *Wiltshire Archaeological Magazine* for June, 1926, page 362, the Editor states that "the whole of the available evidence" is against the presence of large sarsens on Salisbury Plain at any time. Geologists, however, have expressed the opinion that the formation from which the sarsens were derived extended over Salisbury Plain ("*Stonehenge*," p. 71), and as a matter of fact numbers of sarsens are still to be found in that district ("*Stonehenge*," p. 72).

As regards size, it is true that no sarsens as large as the stones of Stonehenge now exist on Salisbury Plain; but this is equally true of the sarsens now to be found among the "grey-wethers" on the Marlborough Downs.

With the means available to the builders, and considering the difficulties to be encountered in gradients and river crossings, the transport of the great stones of Stonehenge from the Marlborough district would have been a most stupendous undertaking. There were 75 of these great stones to be transported, of which some would weigh from 35 to 40 tons. (For details, see "*Stonehenge*," p. 79.)

If, therefore, such evidence as is available were found to be equally balanced between the two sites—Marlborough district and Stonehenge neighbourhood—as the possible source from which the stones might have been procured, we should give the verdict in favour of the latter from considerations of transport. It will be found, however, that, as regards these two alternative sites, the evidence is not equally balanced, but is definitely in favour of the Stonehenge neighbourhood.

The stones for Stonehenge were not got from boulders of the sort now to be found on the Marlborough Downs ("*Stonehenge*," p. 74), but were obtained by splitting up large flat tabular masses of sarsen. The front and back of a Stonehenge stone are thus parts of the upper and lower faces of the original block or slab from which the stone was taken ("*Stonehenge*," pp. 68 and 74).

The largest stone (No. 56, formerly the "leaning stone") is 29 feet 8 inches long over all. It is evident, therefore, that some of the great tabular blocks or slabs from which the Stonehenge stones were quarried would have measured as much as 30 feet or more across, with an average thickness of from 3 to 4 feet. Professor W. Gowland remarks:—

"The sarsens of which the outer circle and the trilithons consist occur "[occurred?] naturally in more or less tabular blocks, generally ranging in " thickness from 2 to 4 feet " (*Archæologia*, vol. 58, p. 75).

There are no such tabular blocks now to be found on the Marlborough Downs, and it appears unlikely that slabs of this description could ever have been formed in that district. Such slabs would have been formed from a comparatively thin stratum of sand in which the process of cementation had been completed throughout from top to bottom. In the Marlborough district the original sand stratum was probably of considerable thickness in which the cementation took place irregularly in the mass, and thus shapeless boulders were formed which were deposited on the chalk substratum when the denudation of the sand bed took place ("*Stonehenge*," p. 46).

The Bagshot Sand Beds, in which the sarsens were formed, appear to have *thinned out* towards the west ("*Stonehenge*," p. 45), and on Salisbury Plain the sand stratum was probably scanty and in patches, lying in shallow depressions on the chalk—hence the scarcity of sarsens on Salisbury Plain compared with the Marlborough Downs. A patch of sand a few feet in thickness would, in the process of cementation, form a flat tabular slab such as has been described above.

The Rev. E. H. Goddard is, however, of opinion that the Stonehenge stones were not obtained from Salisbury Plain, and cites as evidence the fact of the scarcity of sarsens now in existence in that district. (His remarks on the subject are given in full in "*Stonehenge*," pp. 71-72)

On the other hand, Professor W. Gowland, F.R.S., gives opinion as follows:—

"All the [sarsen] stones of Stonehenge occurred in its neighbourhood, within a radius of not many miles, and had not to be brought from a distant locality" (*Archæologia*, vol. 58, p. 75).

Dr. Herbert H. Thomas, Petrographer to the Geological Survey, remarks:—

"The area from which the Stonehenge sarsens were collected need not have been large, and in all probability centred upon the site of Stonehenge itself" (*Antiquaries' Journal*, vol. 3, p. 242).

As regards the scarcity of sarsens on Salisbury Plain. An examination of the stones at Stonehenge would appear to show that the builders were unable to obtain sufficient material of a suitable quality and of a large enough size properly to fulfil their requirements. They had to take what they could get rather than what they would have desired. This indicates a very limited supply.

If the Stonehenge stones were obtained from sarsen deposits in the neighbourhood, we may therefore conclude that these deposits were scarcely sufficient to provide for Stonehenge, leaving over a not very large quantity of smaller boulders, rejections, and broken pieces. These "leavings" in the course of forty centuries, lying in a stoneless district, would not be likely to remain. They would be used up for various purposes, and, when none were left, Stonehenge itself (having by then lost its interest or sanctity) would begin to be demolished.

With the above-noted facts before us, we may consider it probable that the sarsen stones for Stonehenge were obtained from the district in the immediate neighbourhood. If this be admitted, we may further consider it probable that the erection of this magnificent structure was suggested by the proximity of this suitable material.

E. HERBERT STONE.

Australia: Religion.

Radcliffe-Brown.

Arrangements of Stones in Australia. By A. R. Radcliffe-Brown. 133

In the *Journal of the Royal Anthropological Institute*, January-June, 1925, Professor Wood Jones writes on certain ordered arrangements of stones present in certain parts of Australia. The marking out of grounds for ceremonial purposes is common in many parts of Australia and is probably universal. It may be done by temporary arrangements of bushes, or by somewhat more permanent methods in which earth or stones are used. So far as my present knowledge goes these ceremonial grounds are used either for initiation ceremonies or for totemic ceremonies. Those in the eastern parts of Australia seem mostly to have been made for the purpose of ceremonies of initiation, which are held at these spots at intervals of years. On the other hand, in the region where we find the western type of totemism special ceremonial grounds are used for the localised totemic ceremonies that are characteristic of the western type. This western type of totemism is found from the western coast, northwards of the Gascoyne River, to the Gulf of Carpentaria, and eastwards as far as the Dieri, who combine this type of totemism with patrilineal descent, with the eastern type with matrilineal descent. Wherever it is found there are special totemic centres (*talû* in Western Australia) at which ceremonies for a particular totem are regularly performed. At such centres there is sometimes a natural feature, such as an outcrop of rock, a cave, or a tree or trees, which play some part in the ceremony. Sometimes

a temporary structure of bushes may be made for the ceremony. Very frequently the ceremonial ground is marked out by heaps or lines of piled up soil, or by heaps and lines of stones. Each ceremonial ground has its own particular arrangement and plan, no two being exactly the same, though certain general principles recur with some frequency. A smooth clay pan with lines of stones is one arrangement that I have noted in western Australia.

An essential feature of the western type of totemism is the belief in totemic ancestors who established the totem centres and first performed the ceremonies. It is the ancestor of ancestors associated with a particular totem centre who are supposed to have made the ceremonial ground, whether that consists of a natural object such as a rock, or an artificial arrangement of earth or stones.

It would look, therefore, as if some at least of the sites mentioned by Professor Wood Jones were such totemic ceremonial grounds, and that the natives were trying to explain to him what they quite honestly believe, that these arrangements of stones were made by the totemic ancestors, whom Professor Jones has taken for "another race." In some parts women are not allowed to approach the totemic ground too closely, though in some parts of western Australia this would seem to apply only to certain totems such as the rainbow serpent (the *wollunqua* of Spencer and Gillen), and in some of the ceremonies women actually take part, particularly women who have borne many children.

The statement given to Professor Jones by a white man who had lived with the natives should, I think, be treated with scepticism. It repeats the idea common amongst white men in the "back-blocks" that the origin and purpose of the operation of subincision is to limit the number of conceptions in the tribe. Many white men who have been in contact with the natives who practise this rite will tell you quite confidently that this is so. But I have never been able to find a native—even a sophisticated native—who advanced this explanation. We know that in many regions where subincision is practised the natives believe that impregnation is not a physical but a spiritual process, just as death, for the same reasons, is regarded not as a physical (natural) but as a spiritual (magical) event. Even animals and plants will not increase unless, by the proper performance of a totemic ceremony, the germs of the totem species are dispersed over the country from the totemic centre in which they are present. Similarly, the human births are dependent on the proper performance of ceremonies of the baby totem (*ratapa* of the Aranda), by which the germs that cause impregnation are dispersed from the centre of the baby totem. A statement from a white man of the Australian Bush which begins by positing that the purpose of subincision is to limit the population is *ab initio* suspect. After the careful inquiries of Roth and Spencer and Gillen we might have expected to hear no more of this popular theory of subincision in scientific literature.

It would seem highly probable, therefore, that the arrangements of stones observed by Professor Wood Jones are simply totemic ceremonial grounds such as are to be found in vast numbers over a large area of Australia and that the native informants tried to explain to him that they had been made by the totemic ancestors.

A. R. RADCLIFFE-BROWN.

Fiji: Sociology.

Hocart.

Limitations of the Sister's Son's Right in Fiji. By A. M. Hocart, M.A. **134**

In my "Chieftainship and the Sister's Son in the Pacific,"* I suggested that the right of the sister's son to seize his uncle's property was originally a sacrificial custom which became extended through the equation chief—god.

* *American Anthropologist*, 1915, pp. 641 ff.

It was, therefore, at first a chiefly custom, that gradually spread in an attenuated form to all the people in the greater part of Fiji.

But not in all. In the course of writing out my notes on Vanua Levu, I came across the following statement from Nambuna, a village of the Ndreketi-Wainunu tribes: "No ordinary man (*tamata vale*) may exercise the nephew's right here; "only a nobleman (*turaka*)."^{*} All the instances of *vasu* they gave were great noblemen.

These people are more matrilineal than patrilineal. They say that before the Government the mother was more important than the father. If a man goes to stay in his father's land they say, "He has gone to stay in his 'land of arising'"; but if he goes to his mother's, "He has gone to his true land." Of old the true owners (or natives, *i taukei*) of the land were the children of the "woman." A man is "son of arising" (*luve ni thandra*) to his father's land. The sons of a commoner and a lady are gentlemen; but the sons of a gentleman and a common woman are only *luveni* (probably short for *luve ni thandra*). Of the two exogamous groups (*vosa*) into which society is divided, a man follows his mother's. A man is put down to his mother's or his father's clan according as he is living with one or the other. This is true of the whole of Fiji, but in most of Fiji a man usually lives with his father's people, and is not entirely at home in his mother's; but here the cases in which he follows the mother's are at least as numerous. He can plant either in his father's or his mother's land. It is difficult to reconcile this with the prevailing theory that the custom of *vasu* is the assertion of the nephew's lost right of exclusive inheritance. If that theory were correct, we should not find the right among matrilineal people, or a people where the nephew is always potential part-owner of his mother's land, and *de facto* whenever he chooses. He should only exercise the right when he lives with his father's people; but he does when living in "his own true land," viz., his mother's, of which he is "the true owner." To take a concrete instance, Ratu Isireli, a very big man in Ndreketi, is the son of a lady of Ndreketi, and lives in Ndreketi. He is "native nephew" (*vasu i taukei*). "He takes things; but he will not do it every day: it is not as if he were senseless. He only does it when in need." It is among these people I was told the right of *vasu* "is not an everyday thing, but only for potlatches."^{*}

The wholesale plundering which alone appears in the literature on the subject only occurs when a man goes on a visit to his mother's country, his mother being a lady of high rank. Now, such a visit is never anything but ceremonial, and never takes place without a potlatch.

M. Georges Dumézil, in his "Le Festin d'Immortalité" (Paris: Paul Geuthner), has traced the stealing of the sacrifice, viz., ambrosia, and the conflict of the gods and demons, throughout the countries of Indo-European speech. He ends by suggesting a connection with American potlatches. It is interesting that at the same time, working independently and starting at the other end, I should have tried to link up Fijian potlatches with Indian sacrificial customs, and ultimately with the conflicts of Indian gods and demons, "both descended from Prajāpati." See "Maternal Relations in Indian Ritual," MAN, 1924, 76; "The Cousin in Vedic Ritual," *Indian Antiquary*, 1925, p. 16.

A. M. HOCART.

^{*} I have hitherto translated *solevu* by "festival," "ceremonial exchange"; but neither is satisfactory. "Potlatch" better expresses the idea, besides suggesting a common origin, which is probable.

Africa, East: Religion.

Murphy.

The "Kithito" at Mivukoni, Mumoni District, Kenya Colony.

By J. H. B. Murphy.

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This photograph (Fig. 1) shows the famous "Kithito" at Mivukoni, Mumoni District, Kenya Colony. It is the most powerful "Kithito," and is held in the greatest awe not only by the Akamba but by the Akikuyu. It takes seven days to act, and there is no possible medicine procurable to remove the death penalty which overtakes anyone swearing falsely thereon.

The ingredients are contained in an earthenware pot. The only one recognisable is a human tibia. When not required the "Kithito" is kept guarded by a snake in a crevice of the rocks shown in the photograph.

The youth shown has inherited it from his father, and does not appear too well pleased with his inheritance. Although he is wealthy, he can keep no stock—the chief joy of all Akamba—as his association with the "Kithito" kills any cattle that come into his possession.

J. H. B. MURPHY.

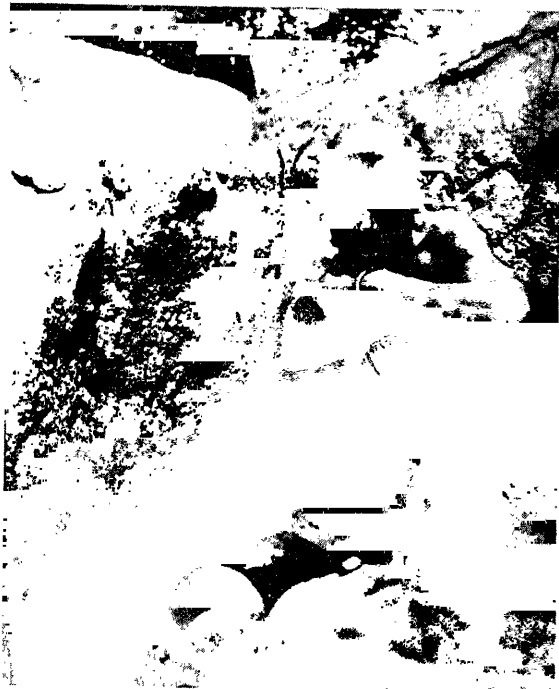


FIG. 1.—"KITHITO" AT MIVUKONI.

REVIEWS.

History. Bury; Cook; Adcock.

The Cambridge Ancient History. **136**
 Edited by J. B. Bury, M.A., F.B.A.,
 S. A. Cook, Litt. D., and F. E. Adcock,
 M.A. *Volume II. The Egyptian and
 Hittite Empires to c. 1000 B.C.* Pp. xxv +
 751, 15 maps and 6 plans. *Volume III.
 The Assyrian Empire.* Pp. xxv + 821, 15
 maps, 2 plans, and 5 synchronistic tables.
 Cambridge, at the University Press (1924
 and 1925). Price 35s. net each volume.

With such an array of distinguished names in its list of authors this work could not fail to attain that degree of excellence that we find in it, and these volumes fall in no way behind the high standard set by their predecessor. Since they deal to a great extent with periods about which there is much written record in existence, the chapters are more definitely historical in their treatment than were most of those in the first volume; less depends on archæological evidence and there is not so much of purely anthropo-

logical matter. Though more stress is laid on political history, these chapters have great value for the anthropologist, and some of them contain much interesting matter on the religious views and practices of the peoples of antiquity.

The first two chapters of Volume II are by the Master of Emmanuel, who discusses the peoples of Asia Minor in the light of the opinions of Herodotus and other Greek writers, and of such evidence as can be gleaned from their languages and place-names. He makes no reference to the writings of Von Luschan or of other anthropologists who have studied this region, and he is inclined to give credence to the suggestion that the characteristic form of the Armenoid skull was due to tight bandaging during infancy. In the second chapter he discusses the peoples of Europe, and, though he gives a brief paragraph to the principal physical types recognised by anthropologists, he treats the subject by that linguistic method to which we were

accustomed during the last quarter of the nineteenth century. The six chapters by Professor Breasted on the history of Egypt are pleasantly written, but mainly political in outlook, while Professor Peet's chapter on the life and thought of that country contains a few paragraphs on magic and spells.

In his chapter on Assyria Mr. Campbell Thompson has some interesting remarks on the influence of the horse upon the foreign policy of the times. He seems to doubt that this animal was introduced by the Kassites, because it is mentioned in the time of Hammurabi, though the Kassites were near neighbours of the Babylonians at that date. He also doubts that this beast was introduced into Egypt by the Hyksos, who arrived there, he thinks, about 1800 B.C., while the Kassites conquered Mesopotamia, he believes, in 1746 B.C. On the other hand, the first evidence we have of the presence of the horse in Egypt is at the close of the Hyksos period, while the invasion of these shepherd kings is placed by most authorities at 1788 B.C. or later.

The chapter on the Hittites of Asia Minor by Dr. Hogarth is a very important piece of reconstruction, as is his later chapter on the Hellenic settlement in that region. Dr. Hall has interesting chapters on the Keffians and Philistines, as well as upon the art of the Near East. Dr. Cook deals with the political history of Syria and the rise of Israel, Mr. Wace with Crete and Mycenæ, Professor Bury with the Achæans and Troy, and Mr. Wade-Gery with the Dorians.

A composite chapter on the Western Mediterranean brings us again into the realm of archaeology. In the section on Italy and Sicily, Professor Peet gives a summary, based largely on his valuable volume. He seems, however, not to have kept abreast with recent opinion on the Epipalæolithic Age, especially in Spain. Dr. Ashby sums up the evidence in Malta, Sardinia, Corsica and the Balearics. Mr. Leeds deals with Spain, where he supplies the links missing in the first section, and gives his views on the origin and spread of megalithic culture, which have already appeared elsewhere. Professor Peet deals, in a very summary fashion, with the pre-history of France and the British Isles. The three last writers are responsible for a joint section on the megalith-builders, in which they fail to come to any agreed solution. The volume closes with an admirable chapter on the religion and mythology of the Greeks, from the pen of Professor Halliday.

Volume III is still more devoted to political history. Mr. Sidney Smith has

five chapters on Assyria, one of which deals with the social conditions of that people in the time of Ashurbanipal; Dr. Hogarth presents two more on the Hittites, dealing to some slight extent with their art and religion; while Professor Sayce contributes an interesting chapter on the Kingdom of Van.

Dr. Ellis Minns has written a peculiarly helpful chapter on the Scythians and northern nomads, in which he draws an admirable picture of the steppes and steppe-life, while he discusses the origins and affinities of the Scythians and describes their religion and burial customs. Mr. Campbell Thompson has two chapters on Babylonia, in the second of which he treats of their literature and science. Dr. Hall deals with the last days of Ancient Egypt and Saite art, and Professor Macalister describes the topography of Jerusalem.

Dr. Cook has contributed four very interesting chapters on the Israelites and their religion, while Dr. Hogarth has given us a valuable chapter on Lydia and Ionia. The growth of the Dorian states is discussed by Mr. Wade-Gery, early Athens by Dr. E. Gardner and Dr. Cary, while the latter contributes also a chapter on Northern and Central Greece. Professor Myres has given us a brilliant chapter on the colonial expansion of Greece, while the growth of the Greek city-state has been traced by Mr. Adeock. This is but a brief summary of the contents of these remarkable volumes; to deal with them adequately would require a volume and the services of a host of reviewers.

H. J. E. P.

Africa, North : Ethnography. Hofmayr.

Die Schilluk : Geschichte, Religion und Leben eines Niloten-Stammes. 137
Nach P. Banholzers, F.S.C., und eigenen Auszeichnungen dargestellt. Von Wilhelm Hofmayr. ("Anthropos" Ethnologische Bibliothek. Internationale Sammlung Ethnologischen Monographien, Band 2, Heft 5). Pp. xvi + 521 + 35 Tafeln. (Mödling bei Wien : Verlag der Administration des "Anthropos," St. Gabriel, 1925). 20 gold marks.

This volume constitutes a valuable contribution to the ethnology of the Nile valley, for, though the Shilluk have been more written up than any other tribe, no attempt had been made by previous writers to provide a detailed account of their social organization, or to give a critical account of the history of their kings and to record the chief events in the reign of each. Moreover many of the habits and customs of the Shilluk had remained unrecorded; in fact the time was ripe for a monograph of this interesting

people. This is not to say that Father Hofmayr's account is to be looked upon as final and complete, for even from the reviewer's limited experience he can produce on the religious side an important ceremony to Nyakang to protect the growing crops, unrecorded by the author, and the account of the everyday life of the people is somewhat lacking with regard to the privileges and functions of the kin and their relatives-in-law. The reviewer has indeed the feeling that the author may have handicapped himself somewhat by following the notebooks of the late Father Banholzer (whose mission was at Lul, not far from Fashoda) too closely, for it will probably be found that there are differences between the Northern and Southern portions of the Shilluk kingdom, indeed, at one time the two halves waged fairly determined warfare on each other. Nevertheless it is certain that this work will long be the standard authority, and it is unlikely that it will ever be superseded on the historical side. Here one of the most interesting facts is the relation of the Shilluk to the Jur, for Father Hofmayr's account leaves little doubt that there was once a "Jur-Shilluk" people and that the proximate area of origin of the Shilluk is the present day Jur country in the Bahr el-Ghazal Province, the country of Dimo of the legends. Here Nyakang, the leader and culture hero and first king of the Shilluk, lived in peace with Dimo his half-brother's son until the enmity of the latter drove him and his followers away. Thus began the migrations that gave rise to the Shilluk nation, while Dimo and his descendants continued to rule the people now commonly known as Jur, who still speak a Shilluk dialect, though their customs at the present day are for the most part, if not entirely, those of their mesaticephalic neighbours.

Coming to later times and bearing in mind his excellent native authorities, it seems desirable that Father Hofmayr's list of Shilluk kings should be accepted as authoritative as against those of previous writers (including the reviewer), while full value should be given to his additions to our knowledge of the life of the king—who, it will be remembered, incarnates Nyakang—for these are both numerous and important. In this connection there is one problem of great interest which the reviewer failed to solve and on which he thinks Father Hofmayr has failed to shed any satisfactory light, namely that of the Ororo, a social group, upon which falls the responsibility of killing the king and which also takes a prominent part in his investiture. Father Hofmayr agrees with

the writer that the Ororo are the descendants of King Ochalo (Ocwolo); but while the writer got no further than this, Father Hofmayr gives an account of a ceremony of degradation that may—so he states—be put upon the descendants (*kwaret*) of any king to reduce them to the condition of Ororo, and gives an account of one such attempt, yet admits that the only Ororo known historically, or existing at the present day, are the descendants of Ochalo. Yet, even if the account of the Ororo cannot be regarded as final, it constitutes a valuable starting point for future inquiries.

The sections on religion are among the best in the book, and present some of the most stimulating reading in African ethnology. With them may be linked the valuable corpus of songs towards the end of the book, since many deal with God (*Jwok*) or Nyakang, and are essentially religious.

Father Hofmayr's researches indicate that there can no longer be any doubt that the Shilluk—at least in the South—are totemistic. It is, however, probably true that while the rules of exogamy are generally observed that side of totemism which emphasises the blood relationship between groups of men and animals plays but a small part in the emotional life of the Shilluk, which, broadly speaking, seems to be so nearly monopolised by the cult of Nyakang as to leave but little room for any considerable development of even such common African traits as ancestor worship (other than the ancestors of the royal family), which here seems reduced to a minimum.

It has not been possible in the limited space available to touch on more than a few of the subjects treated in this important work, and if this note (for it is no more) should appear unduly critical, the writer would plead that it is just the more difficult subjects, and therefore perhaps the more controversial, that are the most interesting and the most worthy of discussion.

The volume closes with a number of photographs and technological drawings. There is no index, but a useful table of contents is provided. C. G. S.

Prehistory: Linguistics. Childe.

The Aryans: a Study of Indo-European Origins. By V. Gordon Childe, B. Litt. (Oxon). The History of Civilisation Series. London: Kegan Paul, Trench, Trubner & Co. (1926). Pp. xiii + 221, 8 plates, 28 figures and 1 map. Price 10s. 6d. net.

Many of us are old enough to remember the time when the Aryan question was much before the public eye in this country. At that time students of Comparative Philology were satisfied that they had correctly reconstructed the early history of Europe and parts of Asia without the aid of Prehistoric Archaeology, and they discoursed much of races and their cradles in supreme ignorance of the elements of Physical Anthropology. In due course these other sciences established their claim to a hearing, British Philologists abandoned the discussion of the origin of the Aryan race, and for nearly forty years such studies have been neglected in this country.

Nevertheless, though abandoned here, research on these lines has been active in Central Europe, where much fresh light has been thrown on the problem of linguistic origins. In the meantime, the study of Prehistory, especially in its later phases, has advanced at a rapid pace, thanks to the discoveries at Knossos. It was high time, therefore, that a work should appear in English giving the latest views of Continental students of linguistics, and at the same time discussing these results in the light of Prehistoric Archaeology and Ethnology.

Such a volume is before us, written by our Fellow, Mr. Childe, in which we find the latest conclusions of European research and much shrewd argument. The author has proceeded by two methods, inductive and deductive, and he trusts to the convergence of these two lines of research to prove the correctness of his thesis. That they do converge is undoubted, but the critic may reasonably surmise that the writer, when selecting from a number of alternative hypotheses, has chosen that which best assists this convergence. Nevertheless he has produced a theory which seems to explain all the facts, and, though it may not solve all the problems, it will undoubtedly prepare the way for their solution.

After an introductory chapter our author cites the evidence for the first appearance of Aryan-speaking peoples in each area, and endeavours by means of archaeological evidence to trace them to their earlier homes. The most masterly is his treatment of the problem in Greek lands. Here he traces their advent to the third period in Thessaly and to a later intrusion into the Spercheios valley. Later he considers the possibility of the primitive Aryans being the makers of the painted pottery, and after a most thorough inquiry dismisses this as a possibility on grounds which will appear adequate to most of his readers. On these grounds he dismisses also the

possibility of the arrival of an Aryan tongue into Thessaly, with its second civilisation. As regards the pottery, this is doubtless true, but the presence of the highly fortified camp at Dhimini and the fact, which he notices, that fortified camps accompany their arrival in the Danube basin, makes it possible that this intrusion into the Thessalian plain was led by Aryans accompanied by makers of pottery from Rumania. Since even the first settlement at Cucuteni was fortified, it is possible that they had obtained a footing there also. After dismissing the makers of painted pottery as non-Aryan, he discusses the racial affinities of the early Indo-Europeans, and by a process of elimination concludes that they were in the main Nordic and that the ochre-grave people of the South Russian kurgans were among their number.

Mr. Childe is unable to make up his mind as to the extent of the Aryan cradle. He admits that the Russian steppe was within it, but he feels that it might have extended to the Baltic region. All depends on the dates of the ochre-graves, which, he thinks, must have had a relatively short duration, although the practice had been in vogue in Upper Palæolithic times. Another point about which he is perplexed is the origin and distribution of perforated stone battle-axes, which he rightly considers the hall-mark of the Nordic Aryans. Did they pass from south-east to north-west, or in the opposite direction, and are their prototypes to be found in Mesopotamia or Maglemose?

The solution of these problems, and the kindred problem of the origin of the *Megaron*, should enable us to decide between the alternatives that our author has put before us, and the fact that he has arrayed the existing evidence and indicated the special points on which we are ignorant, will be a great aid to the ultimate solution of the problem.

H. J. E. P.

Near East: History. Moret; Davy.

From Tribe to Empire: Social organisation among Primitives and in the Ancient East. By A. Moret, Professor in the University of Paris, and G. Davy, Faculty of Letters in the University of Dijon. The History of Civilisation Series. London: Kegan Paul, Trench, Trubner & Co. (1926). Pp. xxx + 371, 47 figures and 7 maps and plans. Price 16s. net.

To explain ancient usages by the customs of primitive peoples is not new. This method has been frequently adopted, notably by our late Fellow, Sir William Ridgeway, and by Miss Harrison. It is, however, somewhat novel to trace the

evolution of the whole organisation of states and kingdoms from a primitive condition by such analogies. This is what has been attempted in the volume under review, and, if only partly successful in this endeavour, the authors have produced a work that is both interesting and suggestive.

The two authors have divided the task. In the first part, M. Davy describes totemic organisation, territorial organisation and the growth of individual power, giving numerous instances of each stage of development. He is an ardent admirer of Durkheim, and has cited little that may not be found in the work of that anthropologist; he disagrees profoundly with most of the conclusions of our Fellow, Sir James Frazer. As a clear summary of some of Durkheim's views, arranged in logical sequence, it has considerable value.

The second part contains a general outline of the political history of the Near East from the pen of Professor Moret. Naturally he makes this centre round Egypt, where he traces the gradual amalgamation of scattered tribes into petty states, into a single kingdom, and ultimately, as the result of outside attacks, into an empire stretching to the banks of the Euphrates. This story is intertwined with the doings of Babylonians, Hittites, Kassites and Mitanni, and the whole presents the best summary we have yet seen of the political relations between early states in the Near East.

The chapter on early Egypt is, perhaps, the least successful, for the author has

failed to appreciate the value of many small suggestions that have been made in this country during the past four years. He makes no reference to the discoveries at Badari, and his account of the pre-Dynastic period is very meagre. In dealing with the early ages in Mesopotamia he is scarcely more detailed, and at times his statements are contradictory. Thus, on p. 190, he states that the Semitic peoples were found in Elam, while on p. 199 he says of the Elamites that "their language is neither Aryan nor Semitic." Again, on p. 194, he combats the idea that Arabia "was actually the cradle of all the Semites," while, two pages later, he points out that "Edouard Meyer has shown that the religious, political, and intellectual civilisation of the Semites indicates that they were originally a *folk of the desert*." In spite, however, of these and other inconsistencies and omissions the author's account of the evolution of civilisation in the Near East is both attractive and stimulating.

Our Fellow, Mr. Gordon Childe, has performed his task of translator with great care, and one is glad to record the absence of certain Gallicisms that have disfigured some of the earlier volumes of this series. One cannot help thinking, however, that he has mistranslated a note on p. 161; if the original is, as we suspect, not *amidon* but *amidonier*, it means "bearded wheat" not "wheaten starch." The legends of two of the illustrations in Fig. 4 have been transposed.

H. J. E. P.

CORRESPONDENCE.

Psychology: Dreams.

Rose.

Central Africa and Artemidoros.

To the Editor of MAN,

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SIR,—The very interesting contribution of Mr. A. G. O. Hodgson on beliefs concerning dreams in Nyasaland (MAN, xxvi, No. 39) set me looking for parallels from classical European civilisation. It is well known that the inhabitants of the Roman Empire generally believed in dream-divination, and we have a work by a professional interpreter, Artemidoros of Ephesos (or, as he is sometimes called, of Daldis), who wrote in the second century A.D. and embodied in his book a good deal of the writings of earlier experts in his pseudo-science. From him I quote the following items, which more or less closely resemble Mr. Hodgson's African interpretations.* I cite each dream by the number given it in Mr.

Hodgson's article, and add a reference to Artemidoros by book and chapter, adding the page and line of the best edition, that of Hercher, Leipzig, 1864. There is, I think, no English translation, though a German one is mentioned in Pauly-Wissowa, *Realencyclopædie*, Vol. II, col. 1335.

(1) *Flying*.—"If one dreams that oneself or anyone else is flying in the sky like a bird, the person flying will enjoy long life and good health." Compare Artem. II, 68 (p. 160, 20). "The worst and most unlucky (of dreams about flying) is to wish to fly and not be able to do so, or to fly with the head turned towards the ground and the feet pointing skywards; this signifies that he looks for no dreamer." Other dreams about flying, he explains, are mostly lucky, but they do not appear to have signified long life in particular.

(2) *Fire*.—"To dream of a great bush fire augurs the advance of war." See Artem. II, 9 (p. 92, 16). To dream of a great fire in the sky, he tells us, signifies

* That it was the soul which dreamed was the almost universal belief, in ancient Europe as in central Africa.

war, or else famine. A bush-fire, of course, would not be a common sight in the well-tilled ancient world.

(3) *Climbing*.—This signifies promotion to high rank, in Africa. Artemidoros (IV, 28, p. 220, 5 foll.) tells of a candidate for a magistracy who dreamed that he went down some stairs and was handed a wreath, a common badge of office in antiquity. He supposed that he would be successful, but the dream really meant something quite different, because, as Artemidoros explains, he went down-stairs in the dream and not up. Dreams of going up a mountain, however, are uniformly unlucky (*see* II, 28, p. 124, 15, and II, 68, p. 160, 1 foll.).

(4) *Loss of a tooth or teeth*.—Means (a) loss of a near relation; (b), that the dreamer's wife will bear a lusty son. In Artemidoros (I, 31, p. 31, 17) to lose a tooth in a dream is to lose some member of one's household—which member, depends on which tooth is dreamed to have been lost. Teeth are also connected with children, II, 67, p. 158, 13 foll., though in a different way; to dream of holding teeth fallen from one's mouth means loss of children, by death or otherwise.

(5) *Œdipus*.—Such dreams occur in Artemidoros, but their significance is quite different.

(6) A flooded river means misfortune, generally connected with a law-suit. *See* Artem. II, 27 (p. 122, 20 foll.); such dreams mean "an unfriendly jury, ill-tempered masters, trouble from some-thing violent and loud-voiced." *Cf.* also (9), second interpretation.

(7) To dream of the death of a sick person means his recovery. In Artemidoros (II, 49, p. 151, 8) if a sick person dreams that he is dead, he will be rid of his illness.

(8) A lion signifies a chief, and to dream of being chased down-hill and caught by him means that a chief is plotting against the dreamer. A lion may mean the emperor,* according to Artemidoros, II, 12, p. 102, 12; to be chased by a bull

signifies "great danger and threats from men of high estate"; *ibid.*, line 3.

(10) (12) No parallels.

(13) "To dream of a snake round one leg means that the dreamer will be bound in prison." In Artemidoros, if a dragon wraps itself around one (this seems the general sense of a slightly corrupt passage, II, 13, p. 106, 13), it signifies bondage.

(14), (15). No parallels.

(16) A hearthstone does not symbolise a chief, to ancient notions, and dreams about it had no such significance as they have in Africa; but the hearth-goddess Hestia, if seen in a dream by a king or other potentate, signifies his power, II, 37, p. 143, 22.

(17) No parallel.

(18) "If one dreams that one is catching fish, the dreamer will find a bag of money; but if the fish are of a slippery variety, like mudfish, the dreamer will not be able to keep the money, which will soon be lost or stolen." *Cf.* Artem. II, 14 (p. 107, 18), "to catch many large fish all at once" (or, "fish both many and large," *ἰχθὺς πολλοὺς ἅμα καὶ μεγάλους*) "is a good dream, signifying gain"; *ibid.*, p. 109, 12, "but all fish of the cartilaginous sort (*σελάχιοι*), if of the long kinds, mean labour in vain and unfulfilled hopes, for they slip through the hands."

(19), (20). No parallels.

It will thus be seen that of the twenty dreams cited by Mr. Hodgson, eleven are fairly closely paralleled by Artemidoros. That worthy certainly had not studied the customs of Central Africa, nor are the Nyanja and Chewa tribes in the habit of reading his book or any later work derived from it. The resemblances are due to a parallel development. It is such instances as this of the way in which the human mind tends always and everywhere to think alike which make me somewhat cautious in accepting "distributionist" theories, if not backed by very strong evidence.

I am, etc.,

H. J. ROSE.

* Or a king, βασιλεὺς, has both meanings.

ANTHROPOLOGICAL NOTE.

The following are the meetings arranged for the Edinburgh and Lothians 141 Branch of the Royal Anthropological Institute in the first half of the current session:—

Tuesday, 19th October.—Sir Everard F. im Thurn, K.C.M.G., K.B.E., C.B. (Hon. President of the Branch), on "Fijian Arts and Crafts." (With lantern.)

Friday, 22nd October.—Sir Everard im Thurn will demonstrate on Fijian ethnographical specimens at the Royal Scottish

Museum, Chambers Street, at 3 p.m. The meeting will be held in the small lecture room on the ground floor of the administrative block.

Thursday, 18th November.—The Rev. Ernest Cartwright, of the Regions Beyond Missionary Union, on "The Central African and his Belief." (With lantern.)

Tuesday, 14th December.—E. M. Buchanan, Esq., on "Burma and its People." (With lantern.)



FIG. 1.—PALM CLIMBER, TRINIDAD.



FIG. 2.—PALM CLIMBER, TRINIDAD.

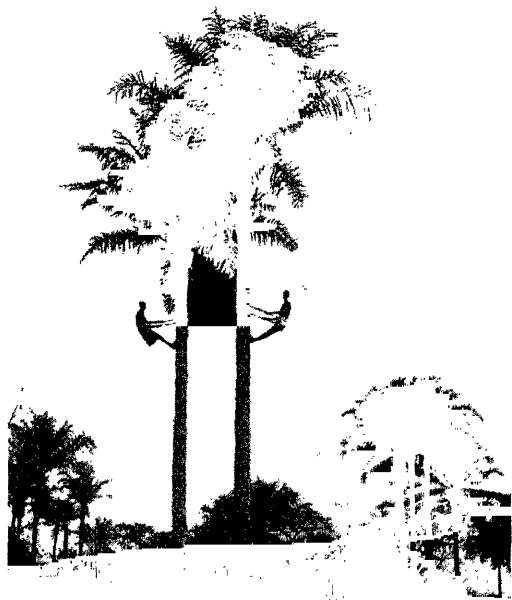


FIG. 3.—PALM CLIMBER, GAMBIA.



FIG. 4.—PALM CLIMBER, GAMBIA.

ORIGINAL ARTICLES.

With Plate N.

West Indies: Archæology.

Russell.

Stone Collars and Elbow Stones. By A. D. Russell. With Plate N. **142**

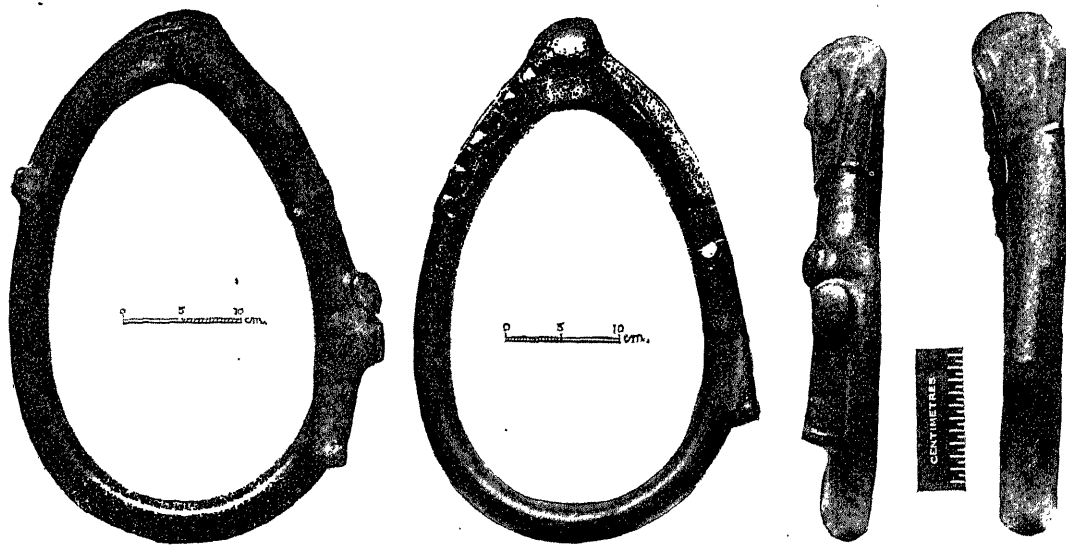
Of all its branches, the symbolism of archæology is naturally hardest. A familiar living object, such as a scarab, or a common utensil, such as a water-gauge, may in itself be recognisable enough; once given a place in ritual, however, and subjected to artistic religious handling, it undergoes a course of progressive ornamentation and idealisation by the end of which any early attempts at realism become taboo, and a conventional emblem, or rather a wide-flung range of them, perplexes the mind of even the acutest gazer. The Nile depth-measure, become part of the religion of two continents, ranges through an indefinite variety of more or less cruciform disguises; the markings of a sanctified beetle turn to Jove's fingers grasping the thunderbolt, to which a little sharpening at the ends now easily transforms its prolonged wings. Books have been written on this subject, which is here referred to merely (*a*) as explaining the fact that collar-stones and elbow-stones have baffled archæologists so long; (*b*) as rendering easier of adoption the view put forward with regard to them by Mr. T. A. Joyce, namely, that they are symbols connected with tree-worship.

Symbols of *what* in connection with it? That point Mr. Joyce leaves undetermined; thus leaving room for the theory which we venture to put forward, not without his kind encouragement, for what the learned may consider it worth.

In the first place, however: *What is a collar-stone?* Though not for students of West Indian archæology, yet for the benefit of other readers, it will be well to state that it is—well, roughly what its name indicates: a stone object in shape much like a horse-collar; but so small, the head of not the most dwarfish breed of horses would go through it; even apart from the fact that in the New World, where these artefacts are found, there was not so much as a cow or a donkey, let alone a horse, down to the time of its discovery by Columbus. No actual horse theory would appear ever to have been put forward. Still, as horse-collars are undoubtedly what have given rise to the name, it may be further mentioned in passing that, even were they larger, still these stone articles would only fit a breed of wry-necked horses. One of the things about them which first strikes the eye is their unsymmetrical shape—bent to one side at the narrower end; a peculiarity which has a not unimportant bearing on one of the two alternative modes of constructing the original mechanism conceived by Mr. Joyce.

Earlier, vaguer, and (be it said without offence) more fanciful theories as to the origin of stone-collars will be found summarised in Mr. J. Walter Fewkes's "Prehistoric Island Culture Area of America" (34th Ann. Rep. Bur. Amer. Ethnol.). Mr. Joyce's alone is thought worthy of detailed consideration. It will be found in the *J. R. Anthropol. Inst.*, XXXVII, 402-419. There the Antillean collar-stone (Fig. 1) is explained as an archaic *zemi* made of branches or other portions of a tree bent into a hoop and fastened at their ends. A specimen in the British Museum is referred to as representing—symbolically, be it always remembered—two meeting ends just at a point where in other specimens occurs what among archæologists has come to be spoken of as a shoulder ridge. If this interpretation of what is seen in the British Museum collar be correct, taken along with the constant appearance of shoulder-ridges in others, it would seem at once to establish Mr. Joyce's main contention, viz., that a wooden mechanism of some sort was represented. "It is perfectly obvious," he writes, "that the collars were constructed originally of wood. A young tree was selected and cut off immediately

"below a fork; the two ends of the fork trimmed into unequal lengths, the longer bent round so as to overlap the shorter, and the two fastened together by a band of cotton." The unsymmetrical outline of such a fork obviously corresponds with the lack of symmetry in horse-collars to which we have before



STONE COLLARS FROM THE ANTILLES (*J.R.A.I.*, Vol. XXXVII, P. LIII, Figs. 1—4).

referred. The fork, however, would not appear to be absolutely essential to Mr. Joyce's theory. Elsewhere he speaks of "a single and comparatively stout stem bent into a hoop and the ends secured by a bandage," which, he says, "may represent a *zemi* made originally from a straight trunk of a tree without a fork."

Put in general terms, therefore, Mr. Joyce's theory would seem to come to, without going beyond, this: that the symbolised mechanism was a wooden contrivance made by uniting ends in a manner corresponding to bosses or projections represented on the stone emblem.

This we conceive to be absolutely and incontrovertibly correct, and our admiration of the acuteness and logicity exhibited by Mr. Joyce in arriving at a sound conclusion in mere abstract terms, is not diminished, but rather increased by the wonder we cannot help feeling at his omission to go somewhat further. Given more familiarity with tropical life, he would, we should be inclined to say, infallibly have done so. Why? Because the mechanism the abstract nature of which he thus deduced from logical premises, is one in common use at the present day and familiar to anyone who has spent any time on a cocoanut estate.

Our suggestion is simple. The mechanism symbolised by stone-collars is the palm-climber, still in common use throughout the world.

Girdle, belt, cincture, would be terms more appropriate than collar; since the thing goes round a man's waist, not round his neck. Wooden, undoubtedly: even in this age, when almost everything is made of iron, we never heard of a palm-climber being so. Formed mainly, to begin with, out of a fork of a tree: that also is highly probable. Secured, most certainly, by the meeting of wooden ends: there, in like manner, Mr. Joyce's acumen seems to us admirably right. His only error—if he be in error at all—lies in apparently conceiving the mechanism to be all of one piece.

The palm-climber is made of two pieces. We speak with knowledge of the mode of its construction alike in the West Indies and in West Africa. In both, two portions of supple wood, such as the cocoanut, the rhun and other varieties of palm supply, are bent round into a long oval hoop, the two ends on the right side being secured by a permanent fastening, those on the left by one easily done and undone, as occasion requires. And—without going into details, in the meagre space here at our disposal—we are satisfied, having examined specimens and illustrations of many stone-collars, that the bosses, ridges, grooves, projections there noticeable, correspond with the various parts in the wooden mechanism as closely as ornamental, conventionalised features are ever likely to do with practical, utilitarian ones. In fact, it seems to us a marvel the correspondence should be so complete as it is.

Comparison of Plates 96 and 97 and Figures 33 and 34 in Mr. Fewkes's work, showing typical forms of the stone collar, with those from Trinidad and Gambia (Pl. N.), photographs of the actual mechanism in use in those colonies, will satisfy the reader on this point better, we conceive, than any lengthy disquisition would do. One word of caution, however, is necessary. In Figures 33, 34 and 37 the mechanism is upside down: the true top and bottom, or, to speak more exactly, the part which goes round the tree and that which goes round the man's waist, will be seen in Figure (b). Three admirable specimens of the stone collar in the British Museum are well worthy of a visit.

With regard to the connection between palm-climbers and tree-worship, suffice it to refer to the tendency, familiar to every student of primitive culture, to regard as divine anything connected with the food supply of the tribe. Under this head, farinaceous food derivable from sago and other palms would be an important item when the chase proved unproductive. Though in certain portions of the New World, such as Peru, the cultivation of cereals was well advanced, in others, such as the Orinoco valley, it probably had no existence; and the conception of a general palm-age preceding the dawn of agriculture is not wholly devoid of plausibility.

Consider now the geographical distribution of the palm-climber, and our identification of it with the stone-collar will be seen to have unexpected results. Among the Jolahs of the Gambia, who use it to tap their wine-palms, its construction is practically identical with that employed in the West Indies on cocoanut estates. How is this to be explained? By the slave trade, it may be said, which brought so many thousands of Africans to the New World. At first sight the explanation seems sound; not, however, when we remember the existence of this very palm-climber in the New World from a greatly earlier period, as proved by its identification with this archaic stone-collar. Unless by the supposition of its double invention in east and west, there would seem to be no escape from the conclusion that it must have passed from one to the other at a period when communication was easier than it became later on owing to changes in the distribution of sea and land, which cannot be dealt with here.

Thus far we have dealt only with stone-collars. How about stone-elbows? it may be asked. Do the same considerations apply to them? In general, we think they do. Their intimate connection with stone-collars seems to be held both by Mr. Fewkes and by Mr. Joyce: neither, however, attempts to determine the nature of the connection. Two alternatives naturally present themselves: the elbow-stone may be (a) a more archaic form of the stone-collar, or (b) it may be a later development of it. We could, were it necessary, say a good deal in support of both views. Not to blurr the outlines of our identification of the stone-collar with the palm-climber, however, we prefer to let such ancillary problems stand over in the meantime. Who knows but some more competent investigator, taking

them in hand, may arrive at conclusions still more far-reaching and unexpected than those to which we have here called attention? A. D. RUSSELL.

Archæology.

Harrison.

The Origin of the Socketed Bronze Celt. By H. S. Harrison, D.Sc. **143**

The accepted theory of the origin of the socketed bronze celt dates back to the publication by Montelius of a "typology of the celt." He derived the implement from the winged celt, with its four flanges curving inwards in pairs over the two faces of the butt or septum. The critical stage in the development is summed up in the British Museum "Bronze Age Guide" (1920):—"With the increase of the wings a central partition or septum was no longer necessary, and disappeared when the socketed celt was rendered possible by the introduction of core-casting." The two semi-circular or crescentic ridges which approach each other towards the middle line on each face of many socketed celts are generally recognised as being based on the curved margins of the wings of the winged celt, and no fault can be found with this identification. Their presence has, however, been a main factor in the widespread acceptance of the view put forward by Montelius, and there are good reasons for subjecting this part of the series to further scrutiny, in support of the objections already raised by Sophus Müller.*

The general assumption apparently is, that the socketed celt arose, on the continent of Europe, from the winged celt, without any influence derived from a knowledge of implements with similar sockets.† That is to say, the single complete socket was here the pure-bred offspring of the double part-socket. It may be that this statement puts the case more bluntly than will be acceptable to some of those who subscribe to the prevailing view, but it is clear that the socket of the European socketed celt either did, or did not, arise independently of all other sockets in the manner suggested.

In the transition from wings to socket, if this occurred by variation, it is evident that more than one link is missing from the series, as represented by actual specimens. The two part-sockets of the winged celt are not only incomplete as regards length and contact, but are open at their distal ends; and the septum, in spite of its approaching dissolution, remains robust. If the vertical socket (as it may be called) arose in the manner postulated, surely there would have been a stage in which two pairs of elongated wings formed an approximately complete double socket, the septum persisting as an essential component. Even when core-casting was employed, the idea of abolishing the septum would not be likely—in the absence of socketed models—to arise and spread with such rapidity that celts with double socket, in whichever way produced, were so rarely made that no unimpeachable example has been recorded.‡ These two missing links are of such fundamental

* *Mém. Soc. Antiq. du Nord*, 1908-9.

† This would appear to be a legitimate inference from references to the transition by Coffey ("The Bronze Age in Ireland," 1913), Wheeler ("Prehistoric and Roman Wales," 1925), and R. A. Smith ("B.M. Bronze Age Guide, 1920"). For Déchelette's attitude see below, p. 218, footnote.

‡ I have been able to find only one record of a celt which was claimed to be a socketed bronze celt with a longitudinal partition. This was the subject of a note, without illustration, sent by Strobel from Buenos Ayres in 1866, and published in "*Matériaux pour l'Histoire de l'Homme*" (Vol. 3, 1867), the specimen being in the Museum at Trent in the Tyrol. It is described as having "une douille parfaite, dans le sens de la longueur, mais divisée en deux par une séparation intérieure. On dirait un paalstab, avec les oreillons unis, d'une seule pièce, formant de chaque côté une gaine." This is the whole of the description, and I have

importance in the evolutionary series that one might be pardoned for calling them the keystones of the arch. Moreover, whether links or keystones, there must have been varieties of the types with double sockets, if variation took its usual leisurely and wandering course. But the winged celt shows little signs of real progress towards a complete double socket, whilst the socketed celt makes its appearance fully-formed. Even the curved ridges on the faces of the socket remain at a discreet distance from each other, as though to proclaim their reluctance to join in bearing false witness. The evidence they offer has, in my view, been misinterpreted. Their occurrence is an illustration of the well-known tendency of man to pay propitiatory tribute to the past, and indicates that a sudden change was made from celts with crescentic curved wings to celts with single socket. If, as I contend, the winged celt became a socketed celt by a mutational* process—by an application of the socket-idea, introduced from outside—it would be natural for the bronze-worker to enshrine in the new type a feature that was reminiscent of its predecessors. If, on the other hand, there was a variational evolution from wings to socket, then the last stage before the making of complete sockets by core-casting would not have been that which was commemorated by the pairs of curved lines, and there would therefore have been no reason for the perpetuation of this particular stage in the development. These lines, in fact, so far from proving evolution by continuous independent variation, tend to establish a probability in favour of mutation, since they suggest a saltatory method of progress; and the jump appears far too big† to have been made without the aid of socketed models.

We may note at this point that in the paragraph from the British Museum Guide, quoted above, the "introduction of core-casting" is suggested as a determining factor in the final stage of the evolution of the socket. But if this device was introduced, surely there came at the same time, or earlier, examples of implements to which it had been applied. The socketed spear-head was apparently known in Europe before socketed celts appeared, and may possibly have played a part in the evolution of the celt.

There is another line of argument which is worth pursuing. Assuming that the socketed celt arose as a result of introduction, it would not be surprising if the adoption of the socket in parts of Europe where the typical winged celt was not the predominant form, was accompanied by the appearance of reminiscent markings of other shapes. This may well be the explanation of the characters of the celts from Denmark, described by Müller, in which the prongs of the haft appear to be

had no reply to a request for further information, directed to the museum at Trent. The record cannot be ignored, though its unsatisfactory character is obvious. The celt may have been of such a late type that the partition was clearly a reversion, and in any case one double-socketed celt is not enough.

Mention must be made of a curious form from Sweden, figured and described by Sophus Müller (*loc. cit.*), which has an imitation of wire binding, used to lash a flanged celt to its haft, cast in one piece with the body of the celt. The haft was, therefore, held in a ring, open at each end and divided into two by the butt of the celt. An essentially similar form, but with two rings and a basal cup, from Ratibor, is figured in "Altschlesien," Bd. 1, Hft. 1, 1922, p. 50. It is difficult to account for the emergence of these anomalous forms, but they have clearly no relation to the winged celt, or to the origin of the socket.

* See MAN, 1926, 101.

† I find it difficult to lay sufficient stress on this issue, which is of fundamental importance from my point of view. There are no doubt some archaeologists who regard the direct transition from the winged celt to the full-blown socketed celt as being well within the capacity of Bronze Age man, without the guidance provided by a knowledge of celts or other metal tools with vertical socket. To me such a view seems entirely at variance with what we know of the originative and inventive powers of man, modern as well as ancient.

indicated.* Another case of equal or even greater interest presents itself. In parts of Central Europe (Hungary, Saxony, etc.) a characteristic form of celt has curved flanges meeting towards the blade, and forming on each face of the butt a part-socket with more or less V-shaped inner margins; from the same region (Hungary) comes a socketed celt in the Horniman Museum which has on each face of the socket a



A

B

FIG. 1.

FIG. 1—A. FLANGED CELT OF A TYPE FOUND IN HUNGARY, SAXONY, ETC.
(said to be from Mainz).

B. SOCKETED CELT WITH STALKED DEPRESSION (ON EACH FACE) WHICH REPRESENTS THE WOOD OF THE HAFT LYING BETWEEN AND ABOVE THE TWO FLANGES; A LASHING OF CORD OR WIRE IS ALSO REPRESENTED. From Hungary (Com. Neutra).

Specimens in the Horniman Museum.

well-defined "mallet-shaped" depression, obviously derived from the V of the flanged celt (see Fig. 1, A and B). That this is not an isolated example may be

* Déchelette's observations ("Âge du Bronze," p. 242, f. n. 2) may be quoted here with advantage:—"Il est certain que le mode d'emmanchement avec douille a été appliqué à " quelques objets de bronze, par exemple à la lance, bien avant d'être en usage pour la hache. " On a donc pu passer parfois directement de la hache à bords droits [flanged celt] à la hache " à douille, comme on le voit par les exemplaires de types spéciaux publiés par M. S. Müller. " . . . Mais c'est l'adoption du modèle à ailerons médians qui dut conduire les fondeurs " à créer le véritable type de hache à douille, au début de la phase IV de l'âge du bronze." It is clear from this that Déchelette attached little significance to the possible, or actual, transfer of the socket-idea from another implement to the celt, as long as it was admitted that the winged celt itself was successfully transformed. It would not be easy to defend this position.

seen from illustrations given by Hampel.* In this case the direct transition to the socketed celt presents fewer difficulties than does that from the winged celt, but the evidence for its actual occurrence by variation is as weak in the one case as in the other.

It appears, therefore, that unless we are prepared to accept the independent, and dubious, evolution of the socketed celt from at least three European types—in Denmark from a flanged celt, and in Central or Eastern Europe from a special type of flanged celt as well as from the winged celt—we must admit the possibility that in all these three cases (potential) progressive variation was cut short by a mutation, evidence being retained in each case of the stage at which the borrowed idea was applied. More than one original European area of reception and diffusion need not be assumed.

The general question of the actual mode of origin and evolution of the vertical socket cannot be discussed here, but it may be suggested that the unpaired fold-over socket such as is met with on certain hoe-blades from ancient Egypt and on spear-heads from Crete, of copper or bronze, offers a much more likely source than the flanged and winged celts of Europe. These required a split in the haft for the body of the butt, whereas the more primitive type was adapted for the reception of a haft that was not weakened in this way. The European forms were specialised types, and the biological "law of the unspecialised" may be applied to human artefacts. Specialised types of implements, like specialised animals, have become committed to a definite line of advance, or to extinction, and it is not from these that new lines of progress are initiated; it is the simple unspecialised forms that remain adaptable enough for evolution to proceed, whether by variation or mutation. The winged celt, arrived at by variation from the first beginnings of the hammered flanges, had gone too far in its own direction for it to develop into the socketed celt by the same process, and although it was saved by a mutation from being the last of its ancestral line, the result was merely a final specialisation—the socketed celt—which, like the bronze palstave, had no future before it. They were "dead-ends," and it is probable, moreover, that their influence in establishing the shouldered haft as the predominant mounting for European axes caused them to be the chief obstacles, during the Bronze Age, to the adoption of the more adaptable types of axe with transverse socket.

In the absence of archæological evidence to prove that introduced socketed implements, and the method of making them, were at the disposal of the identical bronze-workers who produced the first European socketed celts, I cannot pretend to have disproved the accepted theory, except to my own satisfaction. I have done my best to undermine it by laying stress on technological considerations that seem to me to have weight, and by pointing out important gaps and contradictions in the material evidence for independent invention. I am, of course, disposed to look for a single origin of the cast vertical socket, and in so far as this was dependent on core-casting, there seems slight probability that it was arrived at more than once; what little evidence there is seems to point to the Eastern Mediterranean as the ultimate source, perhaps of both the single vertical fold-over socket—originating

* Hampel figures a socketed celt, in the University Museum at Buda-Pest, which is practically identical with my Fig. 1B (see his Pl. XI, Fig. 8). Others illustrated by him are clearly not far removed from the same type, and the ornamental markings on yet others are derived from modification and multiplication of V's and U's which are obviously of the same origin (Hampel, "Trouvailles de l'Âge de Bronze en Hongrie," *Inter. Cong. of Prehist. Arch.*, 1876, Pl. X.-XIII). A convincing series could be selected from these plates, but the links that are missing in the winged celt series are missing in this. I have to thank Mr. Gordon Childe for calling my attention to this and some other papers.

in the bending over of a broad tang or butt of a copper implement—and its derivative, the entire socket cast by means of a core.

It may be said that, even if my views are accepted, I have left the series as it was, and that it still remains true that the socketed celt was derived from the winged celt, in an area where this was predominant. Be this as it may, I have attempted to show that a mutation was probably at the bottom of the most difficult step in development, though I should have no quarrel with those who might prefer to say that the socketed celt was introduced into Europe and there made as an entirely new type, which was given characters reminiscent of the appearances presented by various flanged predecessors when hafted for use.

The foregoing notes may be regarded as an exercise in the analysis of invention, and also as the suggestion of a need for a closer scrutiny of our reconstructions of the development of artefacts. It is especially desirable that there should be an avoidance of the ingenuous recourse to independent invention as an explanation which renders evidence of origin and evolution superfluous. Spontaneous generation has long been discredited, and even the evolutionist must believe in evolution—in moderation.

H. S. HARRISON.

Archæology.

Burkitt.

Archæological Notes. *By Miles C. Burkitt.*

144

It has become more and more usual to employ some word to denote the period lying between Palæolithic and Neolithic times, and during which several apparently disconnected cultures flourished over large tracts of Europe. Under the old classification these cultures were partly grouped as Palæolithic (the Azilian, Tardenoisian, Maglemose, Asturian), partly as Neolithic (the Kitchen Midden and Campignian)—the occurrence of domestic animals and pottery being the criteria used to determine where each should be classed. The creation of a new major period lying between the Palæolithic and the Neolithic is, however, a great convenience, as these intermediary cultures, although largely unconnected, are necessarily closely allied, and the domestic animals-pottery criteria break down. Thus, to class the Maglemose culture as Palæolithic and that of the Kitchen Middens as Neolithic is really absurd, and what is the prehistorian to say in respect to Mughem, where the industry is clearly Tardenoisian, but a little simple pottery occurs?

Objection has been made that the origin of several of these cultures lies back in Palæolithic times, and that therefore they should be classed as Palæolithic. But surely this argument is not valid? Who will affirm that the roots of the Neolithic civilisation do not lie buried deep in the Palæolithic soil in some unknown region? The major divisions are not based on origins.

The limits of the new period can be readily defined. It starts with the rapid change of climate which took place in Europe at the end of Magdalenian times, and finishes with the appearance in quantity of polished stone celts. At various times within this period there roamed about Western Europe folk belonging to several cultures, who influenced one another to a greater or less extent, and who, towards the end of the time, were influenced by the new progress from outside, which was to revolutionise the history of mankind. Living under similar conditions, their existence and desires were, therefore, not vastly dissimilar.

Three names are at present in use for this period: (1) Transitional; (2) Epipalæolithic; (3) Mesolithic. I must admit that I vote for the last of these. Transitional, without explanation as to whence and whither the transition leads, is rather meaningless. Epipalæolithic is too much of a mouthful, and has the same objection as the use of the word Eneolithic for the Copper or Chalkolithic period—it is not a sufficiently distinctive word. Mesolithic is a term which has

none of these disadvantages, and should its use become common the prehistorian will probably find it a great convenience.

In the above note mention is made of a new influence from outside which affected the Mesolithic folk, and later profoundly altered human history in Europe. Was this Neolithic civilisation brought into Western Europe by hordes of invading people, or was it not rather produced by the infiltration of a new body of ideas and knowledge which transformed the Mesolithic folk themselves? The new knowledge included that of agriculture and the domestication of animals, pottery-making and the grinding and polishing of stone implements. Among other things these innovations enabled mankind to increase in numbers, caused community life to develop, and permitted the practice of carpentry. This suggestion does not apply for the Northern Area, where definite immigration of new folk can be traced; but for Western Europe was it not merely the arrival of these fertilising ideas which brought about the change? This would perhaps explain why the Neolithic cultures of Western Europe, except when in contact with the brilliant Mediterranean civilisation, lagged to a certain extent behind those of the North and East. There was little mongrelising of the stock.

The population increased in Neolithic times, but the new knowledge permitted this and the Law of Malthus offers a sufficient explanation. Nature is always breeding a larger population than she can support: if for any reason she becomes suddenly able to support more, that excess of population does not die. This can, of course, be clearly seen in England in the last century with the rise of industrialism. In early times, for Industrial Revolution read Neolithic Civilisation! Already in late Danubian I. times influences from the Lower and Middle Danube area had pressed westwards along the forest-free loess lands and reached Belgium, as can be seen by a study of the decoration motifs on Omalian pottery, which, according to Nandrin and Servais, is really a very early Neolithic culture in Belgium.

It is not suggested, however, that this was the sole channel by which the Neolithic civilisation reached the west, nor is it claimed that none of the new folk ever penetrated our region. But the thesis is thrown out for consideration that the Mesolithic folk played a very large part in the formation of the Neolithic civilisation of the Western Area, and that the increase of population which we find was not due directly to immigration from outside, but was made possible by the absorption of the new ideas.

Interest has been aroused lately in finds made near Vichy, and alleged to be of very early date. During the last two or three years, Dr. Morlet has done me the honour of forwarding a series of brochures that he has been publishing concerning the excavation. I am bound to say I have been sceptical as to a very early date for the finds. I have even been heretical enough to mention the word Gallo-Roman in their connection—very tentatively, however, as it is a period about which, unfortunately, I know very little. It was interesting to find lately that M. Breuil himself was by no means enthusiastic as to the early date of the Glozel finds.

A connection with cultures of prehistoric Crete has been suggested, but surely the site is too far removed from a seaboard; it is near the coast that one would expect influences from seawards to operate. Perhaps prehistorians would be wise to place the whole matter to a suspense account at present until further work of a more definite character has been undertaken. Should it prove that the Glozel finds are really much later in date than has been suggested, it will not detract one whit from their importance, as the objects themselves seem to be of very considerable interest, and would help to elucidate further the story of a not too well-known period.

M. C. BURKITT.

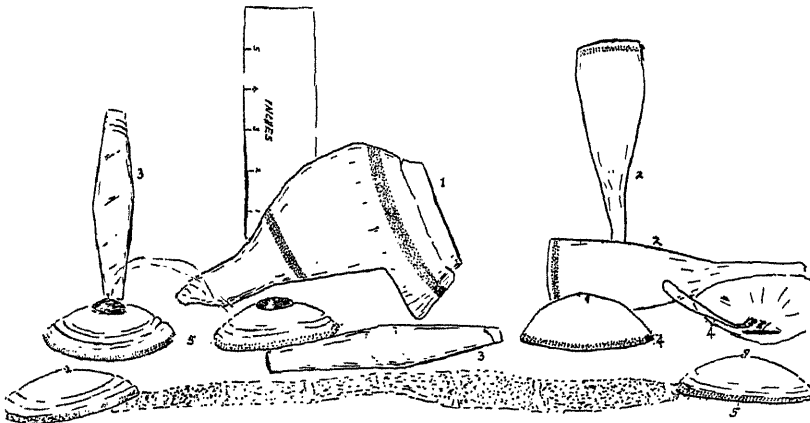
Assam, India, South: Ethnography.

Hutton.

Naga Chank Ornaments of South Indian Affinities. By J. H. Hutton, C.I.E. 145

In the *Journal of the Royal Anthropological Institute* (Vol. LIV—January to June, 1924, pp. 157–165) there appeared an account of a find in some Iron Age graves in the North Arcot district in South India. This find included certain chank-shell objects depicted in Plate XXXII of that number of the *J.R.A.I.* These chank-shell objects were exhibited at a meeting of the Institute in December, 1924; and it was at once remarked by Mr. Henry Balfour that they were more or less identical with ornaments actually in use in the Naga Hills district of Assam at the present time. None of the Naga tribes use all the ornaments referred to; but all, I think, with the possible exception of No. 8 (Plate XXXII), are in use by one tribe or another. The decoration, however, of the Naga ornaments is much simpler than the engraving on the Odugattur finds, and is restricted principally to rows of incised dots, though the Konyak Nagas sometimes use slightly more elaborate forms.

Thus Mr. Richards's chank with the columella cut away (Plate XXXII, Fig. 7) is used by the Angami Naga males, who wear it on the back of the neck,* round which it is fastened by a flat band of black cloth or of very small blue glass beads. This Angami ornament (No. 1 in the accompanying illustration) is sometimes



1. CONCH SHELL (COLUMELLA CUT AWAY) WORN BY ANGAMI AT BACK OF NECK.
2. TRIANGULAR PIECE USED AS END PIECE TO NECKLACE (ANGAMI).
3. AO BEAD (MADE FROM COLUMELLA).
- 4 AND 5. PAIRS OF EAR SHELLS—4. CHANG, 5. KONYAK.

ornamented with a row of black dots crossing usually the upper, but sometimes the lower, end of the shell or both. This ornament is not used, as far as I know, by any other Naga tribe except the Kachha Nagas, who are largely influenced by the Tengima Angami, and some of the Khoiraos similarly under the influence of the Memi Angami of the Manipur State. Nor does the Angami wear any other ornaments corresponding to those from the North Arcot graves, but the Angami village of Khonoma does make for the purpose of trade the cigar-shaped beads (Plate XXXII, Figs. 5 and 6), which are manufactured by rubbing down the columella of the shell into that form. These beads (No. 3 in the accompanying Figure) are worn by the Ao and Chang tribes.† The same Angamis of Khonoma also trade in the pieces of chank shell from which the ear discs worn by the Chang,

* See "The Angami Nagas," p. 23, and illustrations facing pp. 25 and 41.

† Vide Smith—"The Ao Naga Tribe of Assam," illustrations facing pp. 89 and 194.

Phom, Konyak and Yimtsung Naga tribes are made, though they do not produce them in the finished form. These ear-discs (Plate XXXII, Figs. 1, 2, 3 and 4) take different forms in these tribes, the Yimtsung discs being plain, nearly flat and small enough sometimes for a man to wear two pairs, a larger one in the top of each ear with a smaller one below it, each disc being bored in the centre and fastened by a toggle. The Chang, on the other hand, usually prefers a convex disc without a central hole. It is attached to the ear, which it covers, by a loop of string, which goes over the ear and is fastened to a protuberance left for this purpose on the inside of the shell. These Chang ear discs (No. 4 herewith) are often decorated with rows of incised black dots round the edge. The Konyak disc (No. 5) is usually slightly convex—less so than the Chang, and centrally bored. It is ornamented with a similar row of black dots round the circumference and by incised circles round the central hole, which is often covered by a little piece of thin horn-like* *appliqué*, to which the string is fastened. It is usually worn one disc at each end of a pair of strings or of a flat band of very small coloured beads arranged in geometrical patterns with much taste. This band is worn across the top of the head so that the discs fall over the tops of the ears.

In addition to the ornaments mentioned, both the Angami and the Konyak use triangular sections of the main shell, the former as end pieces for bead necklaces (No. 2), the latter as pendants. The Angami decorate theirs with the usual black dots across the broad end, while the Konyak occasionally cut geometrical figures in the centre of their pendants, and a Konyak pendant of this description will be found in the Oxford University Museum.

The conch shells from which these ornaments are made are nowadays all imported from Calcutta or Dacca by the Angami traders of Khonoma, but this trade must, I think, be a recent one, as there can have been no direct communication between Khonoma and Calcutta before 1870, and the supply of such shells was probably very limited indeed. Major John Butler, writing in 1855, records that a male slave is worth one cow and three conch shells and a female slave three cows and four or five conch shells.† As soon as the supply of these shells became, as it has done, plentiful, the value set on them fell, and they have lost a certain amount of their popularity in consequence, though they are still much worn by young men. A reasonably good shell can now be bought for Rs. 5.

Mr. Richards's find at Odugattur was made in a stone grave of dolmen type inside a stone circle. Nowadays, Nagas do not generally build dolmens, but dolmens do occur in the district (*vide J.R.A.I.*, LII, 243, and "The Angami Nagas," p. 19), and in at any rate one Angami (Khezami) village, Khezakenoma, there are dolmen graves, though the usual Angami grave is a stone-lined or wood-lined pit with a cairn superimposed (*vide* "The Angami Nagas," p. 226). Some of the Lhota Nagas bury in a vault cut out of the sloping sandstone rock, with stone slabs fronting and partially roofing the aperture. Some Konyak Nagas use little dolmens to cover the pots containing the skulls of their dead, and the Kaccha Nagas (Lyengmai) of Henima and Intuma showed me dolmen graves in which their ancestors were buried. Such dolmens seem to be no longer used by these villages, but vaults like those used by the Lhotas appear to be still employed. Stone circles are put up to commemorate the dead by both Sema and Kaccha Naga tribes (*vide* "The Sema Nagas," pp. 245, 246), but the dead are not actually buried inside them.

In the *J.A.S.B.* for 1879 (XLVIII, ii, No. 3, p. 141 and Pl. XV) will be found a description of certain polished stone celts from Vellore in the North Arcot

* Made from the covering which a species of large land-snail draws in after it to close the opening of its shell.

† "Travels and Adventures in Assam," p. 157.

district, *i.e.*, from the neighbourhood of Odugattur. The type depicted is identical with a type which does occur, though it is far from being the prevailing type, in the Naga Hills also. The prevailing type of celt found in this district is roughly shouldered, but the Vellore type described by Cockburn, as referred to, unquestionably occurs in the Naga Hills (*vide* "The Angami Nagas," pp. 405, 409; "The Sema Nagas," p. 254, Fig. 5), and a specimen from the Sema country will be found in the Oxford University Museum.

Fergusson* mentions a type of circular stone platform as occurring in deserted sites in the Madras hills in conjunction with abandoned terraces, and this circular platform appears to be identical with the round form of the Angami *baze*, also, inasmuch as it is made by the Angami tribe, associated with terraced cultivation. It is a curious coincidence that a form of grave similar to the round Angami cenotaph called *baze* should be called *bazina* in North Africa.†

It is perhaps worth mentioning that the Angami have a tradition that a section of their tribe split away and went down to the plains and may be recognised by their use of wooden dishes. I do not, however, seriously suggest that the remnant of this schism is to be sought in North Arcot. It seems to me more likely that the Nagas contain an element which has migrated from Southern India across the Bay of Bengal and thence drifted north-westwards across Burma, an hypothesis which would, according to one view of the origin of the Karens,‡ associate this element in the Naga tribes with the Karens of Burma.

REVIEWS.

Ethnology.

Psychology and Ethnology. By RIVERS. Edited with Preface and Introduction by G. Elliot Smith. London. 1926. 8vo. Pp. xxviii, 324. Price 15s. net.

The editor notes that "the difficult task of selecting . . . the series for inclusion in this volume has been made easier by Mr. W. J. Perry's advice," and most of its contents bear more or less directly on theories popularised by Mr. Perry. A few are psychological or psychomedical, but most are grouped under a single heading, "Diffusion." Some are examinations of special problems of ethnographical distribution; others are more general, and it is convenient to have most of the papers which have been much quoted in recent controversy collected. It would have illustrated better the stages by which Rivers came to "relight the lamp of history" in the spirit advocated by Tylor in 1871 (as the editor notes on page xxviii), if they had been arranged in order of date; or if the dates of all of them had been supplied. This could have been more easily inferred from internal evidence, if Mr. Perry had not "collected the bibliographical references for several chapters,"

Rivers.

without indicating which are Rivers's authorities and which Mr. Perry's corroborations. An instructive confession opens the paper on the "Distribution of Megalithic Civilization" (1915):—"Ever since I became interested in the contact and blending of peoples as the dominant factor in human progress, I have expected that the megalithic monuments of the world would provide us with the first convincing demonstration of the importance of these facts." This was not a good frame of mind for a psychologist or anyone else in which to approach a most complicated and obscure problem, not so much of ethnology as of prehistoric archaeology; for, with the exception of the Todas and the Banks Islanders, it may be doubted whether there is any extant people to whom such monuments may be attributed, and whose motives for erecting them may be studied directly. There is always a danger when an enquirer trained in the methods of a systematic and experimental science attacks historical or geographical problems, that he may underestimate the fragmentariness of the evidence, and the perils of hypothesis where there cannot be experimental verification. Rivers, it is true, moved more cautiously than some of those who make most use

* "Rude Stone Monuments," p. 473, Fig. 214.

† Fergusson, *op. cit.*, p. 397.

‡ *Vide* McMahon, "The Karens of the Golden Chersonese," p. 109 *sqq.*, and see also "Man in India," IV, June 1924, "Assam and the Pacific," p. 12 *sq.*

of his suggestions and records; but he seems to have accepted very shaky compilations as basis for some of his arguments, and he allowed himself to generalise from experience gained in a limited group of regions where the kind of "ethnological analysis" which he practised was unusually applicable and fruitful, without going so far in the direction of a "historical" method as to excavate any one of the monuments whose origin and purpose he was examining. The "Druidical" folklore associated with West European dolmens, and the myth of "King Solomon's Mines" about stone structures in Rhodesia, have not necessarily any more to do with their design than the secret societies of the New Hebrides with the local "megaliths"; but it was only by ascertaining that the dolmens were funerary chambers and that the Rhodesian monuments were not yet a thousand years old that the guess that the dolmens were originally "tables" or "altars" for ritual use, or that in Zimbabwe was the palace of the Queen of Sheba, could be refuted. And conversely, if it turns out that the "megaliths" of the New Hebrides were built as accessories of a mystic ritual, it would seem to be futile to go on comparing them with chambered-tombs on the other side of the world just because both are built of large stones. If the proper method be, as Rivers insisted (p. 144), "to seek out the historical antecedents" of the convergent customs," so elementary a precaution as a thorough study of the monuments themselves may fairly be demanded as a clue to the motives of their creators.

The editor's introduction, entitled "Dr. Rivers and the New Vision in Ethnology," contains a characteristic piece of "historical method." On pp. ix-x, "... the problem of the social functions of the mother's brother loomed large in his programme of work. In attempting to appreciate the circumstances in Rivers's own case that led to his adoption of ... as the main interest of ... is curious to note the efforts of his maternal uncle, Dr. James Hunt." ... "When Rivers was three weeks old, his uncle presented to him a copy of" the English translation of Th. Waitz's "Anthropologie der Naturvölker," in which the statement is made that "civilisation is dependent more on historical events than on original mental endowment." In this book "its owner took no interest whatever" for thirty-five years; so we have a "curious" instance both of the diffusion of ideas through the covers of an unread

book, and of the "social functions of the mother's brother" in modern English society: *post hoc, ergo propter hoc*: Q.E.D.

Another odd piece of reasoning is on p. xxvi, where it is said that "the factor that played the chief part in perverting" Tylor "from the essentially diffusionist interpretation adopted in his earlier book ('The Early History of Mankind,' 1865) was the conviction of the universality of what he called animism;" and that "when Dr. Rivers demonstrated that symbolism was not universal, he completely undermined not only the Freudian interpretation of ethnological data, but equally also that of Bastian and Tylor." This argument would only be coherent if there was any reason to believe that "animism" and "symbolism" are identical; but there is no attempt to prove this, and without this proof the argument is in the form "*A is B, therefore C is D.*" Moreover, all that Rivers did in the paper referred to on p. xxv (but not included in this volume, and "quoted" only without quotation marks) was to show that a particular instance of symbolism was not universal. To argue from it thus is to use an "O" proposition as if it were an "E." J. L. M.

Mongolia: Archæology Andrews.

On the Trail of Ancient Man. By Roy Chapman Andrews, Sc.D. 147
New York: Putnam, 1926. 25s net.
Pp. xxiv + 375, with numerous plates.

The Central Asiatic expedition organised under the auspices of the American Museum of Natural History by Dr. Andrews marks an epoch in the exploration of those arid regions. It has been planned on a truly vast scale. Specialists in all the relevant branches of science have been included on the staff; equipment, from the most modern cinematographs to the well-tried camel, has been provided. The expedition has been already in the field since 1921 and campaigns will continue annually till 1928. Fourteen volumes have been projected for the publication of the scientific results.

The book before us does not form part of that series. It is a very pleasantly written account of the first year's work, intended to give the general public some idea of the important results already obtained and of the conditions of life and work in such an expedition. In both objects it is thoroughly successful. Serious readers who are not palæontologists will be glad of the simple and vivid account of the remarkable discoveries of fossil animals and their bearing on the general questions of geology and terrestrial history.

The archaeologist will find his appetite merely whetted by the brief description of the Expedition's finds of prehistoric artefacts. These are not as yet so sensational as the discoveries of extinct monsters. The oldest certain remains of man seem to be some implements said to be akin to the curious series lately laid bare by Licent and Teilhard de Chardin in Ordos, found on a gravelly land surface in the Orok Nor Lake region. Then at Shabarakh Usu two industries were detected, one possibly epipalæolithic without pottery and another with sherds. No details are given as to the precise characters of the implements and it is not always easy to distinguish which industry the author is referring to. A plate showing some selected flint implements, however, serves to supplement this report as well as the slightly more technical account given by Nelson in the *American Anthropologist*. To make up for the comparative paucity of archaeological details, the book gives a very lively account of aspects of Mongol life and thought which will be of real use to the anthropologist. We have every reason to hope that when Dr. Andrews makes his next report he will be able to give us some further clue as to the context of these intriguing lithic industries in the general scheme of prehistory. V. G. C.

New Zealand: Ethnography. Rout.

Maori Symbolism. Report made by Ettie A. Rout from the Evidence of Hohepa Te Rake. London: Kegan Paul, Trench, Trubner & Co., Ltd. 1926. Pp. xxxii + 322. Price 21s. net.

This is a most amazing book. Adapted from the recital of an Arawa chief, it gives a very picturesque account of the alleged origin of the Maori, his migrations, his culture of race and health, and his Sacred Life Symbols. It is of interest in one respect, as a striking illustration of the manner in which European ideas, concepts and beliefs are incorporated into the native scheme of thought, and are then put forward by the Maori of the younger generation as genuine old-time traditions. But as a scientific treatise it is on quite another plane.

One sympathises with the authors in their desire to dispel the murky atmosphere of sensationalism which has enveloped so much of the non-scientific interest in native peoples. Of the high intellectual standing of the Maori, of his insistence on bodily fitness—a point adequately touched upon by Sir Arbuthnot Lane in the preface—of his obedience to the voice of the community and the duties of hospitality there can be no question.

But, as stated in the introduction, the aim of the book is to reveal the worth of the Ancient Maori civilisation, and “to counteract the many foolish and disgraceful untruths through which Maori culture has hitherto been so deplorably misunderstood.” In the face of this yearning for truth and accuracy, it is somewhat distressing to read that “the traditional descent of the Arawa Maori is from I-Haka (Isaac), who was the son of A-Pera-Hama (Abraham),” (p. 15), and that according to the Maori legends, “as handed down for many generations by the Elders and Men of learning . . . the Holy Land, the land where Mankind (*Maori*) was created, was Assyria (*Irihia*)” (p. 3). Or that in pre-European days “Tobacco and rows of sugar-cane were also cultivated in the woman’s gardens; sunflowers were grown abundantly for beauty as well as for bird food. Maize, also used for feeding birds, was raised with the general crops” (p. 76), and again that “Reading, writing and arithmetic were known; correspondence was carried on by carrier-parrot, and there was a public postal service by male couriers who carried inscribed wooden tablets” (p. 83). After this the use of the water siphon to irrigate fields, flag-signalling, tattooing as a form of writing—with elaborate “translations” of the same—the Turtle as the Arawa totem, the casting of statues and megaliths from molten lava, and countless other equally wild assertions, do not come with so great a shock. It is, however, with mild surprise that one learns that “Charles Darwin and Alfred Russel Wallace were indebted to Maori lore for their doctrines of Evolution by natural selection, sexual selection and the survival of the fittest”! (p. 97). These statements, chosen at random, give sufficient indication of the calibre of the book.

The illustrations, selected by Miss Rout, are in themselves an admirable essay in the art of constructing uncritical diffusionist hypotheses. With the Maori “Sacred Legends,” which, in their testimony to the unity of civilisation, record the building of the megalithic structures in Mexico and in Peru, and of the pyramids in Egypt, we must leave others to deal as they think best!

R. W. F.

Abyssinia: Ethnography. Darley.

Slaves and Ivory: a record of adventure and exploration in the unknown Sudan, and among the Abyssinian slave-raiders. By Major Henry Darley, with an introduction by Charles W. Hobley, C.M.G. London: H. F. & G.

Witherby, 1926, pp. xvii, 219; photographs and maps.

In simple straightforward style Major Darley tells a story of amazing adventures in Abyssinia and the no-man's land between that country and the Sudan. The book is written as an indictment against the slave trade and the British policy of treating the Kenya-Sudan boundaries as demarkating closed areas, *i.e.*, closed to all except Abyssinian raiders. There are also some bitter remarks about the unfortunate first appearance of the white man and his beneficent government, phenomena which the untutored natives ignorantly mistake for particularly efficient cattle raids.

There is very little ethnology in the book, and that is not always reliable—*vide* the remark on page 15 that "the Dinkas, the Rotillas, the Toposans, the Turkana, and numerous other sub-tribes . . . speak 'different' 'patois' of the Masai language." However, Major Darley's actual observations are always interesting and ring true, even that hardy perennial, the story of the chief's son who brings his ancient father to the white man requesting him to kill him, because he has heard that the Government now holds the monopoly of slaughter and regards the good old custom of killing the "king" by his own people with disfavour. This time the story comes from Gebel Lafon, and may be yet another instance of the sacred rainmaker king, so prevalent in this part of Africa.

Perhaps most interesting of all is the account of the condition of affairs in Abyssinia on the death of King Menelik, and the Abyssinian assumption that similar conditions must prevail in the British Empire on the death of King Edward. These included concealment of the actual fact, withdrawal and replacement of every Government official, and an interregnum of complete lawlessness.

BRENDA Z. SELIGMAN.

Anthropology, Physical
Degeneracy.

Estabrook:
McDougle.

Mongrel Virginians: the Win Tribe. By Arthur H. Estabrook and Ivan E. McDougle. Baltimore: Williams & Wilkins Company, 1926. Pp. 205 + 2 pls. Price 13s. 6d. net. **150**

This book, which is written with a professed eugenic aspect, is somewhat misleading. It is a study of a certain more or less inbred community in the origin of which the White, American Indians, and Negroes have participated. They have, possibly because of their origin, but largely because of the social environment, become isolated, and forced into a more or less degraded status. That a large number of this community are reported to be unworthy and profligate, does not seem any ground for the assumption so calmly made by the authors that their defects are due to racial intermixture. The example of the Jukes family, so frequently quoted in this book, is a lesson which seems not to have been appreciated by the authors, *viz.*, that a white-raced family may produce an even more degenerate stock than that of the Win tribe, which has been so methodically studied by the authors.

This work cannot be taken into serious consideration from the point of view of physical anthropology. No term is defined. Persons are spoken of as being "typically Indian"—whatever that may mean, and the term "colour" is used in an equally indiscriminate manner.

Like so many books on matters eugenic, this suffers severely from a want of scientific method. How much better it would have been to have studied intensively a few of the recent matings in this group, and to have accompanied it with accurate measurements, photographs and, above all, with adequate intelligence tests—*desiderata* wholly wanting in this work.

R. N. SALAMAN.

CORRESPONDENCE.

Ethnology.

The Children of the Sun.

To the Editor of MAN.

SIR,—Mr. J. H. P. Murray has mentioned, in MAN, 1926, 70, some of the difficulties encountered by him in reading "The Children of the Sun." His criticism is welcome, for it serves to draw attention to omissions and to mistakes in points of detail that are bound to occur in a wide survey of an unorganised body of facts.

The criticisms are mainly concerned with the distribution of gold, pearls and remains. I must remark that I relied

Perry

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almost entirely for my evidence with regard to distributions on Chinnery's paper, "Stone-Work and Gold-Fields in British New Guinea" in the *Journal of the Royal Anthropological Institute*, XLIX. My map was based on Chinnery's. His main thesis was that certain remains found in parts of Papua and New Guinea were left behind by a people who were attracted there by gold and pearls. I agree with him that no other interpretation can be put on the evidence.

I will take the criticisms in order as they are made, the numbering being that of Mr. Murray.

(1a, b). I did not mention pearls in the Trobriands because I did not know of their existence in that group.

(1c and 2). Polished Stone Implements. The places indicated on the map are those of the occurrence of implements of types not made by the natives. They certainly are not "carefully located alongside the gold and pearls" by Chinnery or myself. This correlation of distribution has to be explained by those who do not agree with us. On the other hand, when I wrote I was unaware that the manufacture of polished stone implements still persisted in Papua, although I knew of it in Geelvink Bay. The point, of course, is that the implements shown on the map are of types unknown to the natives, and therefore not made by them.

(1d). See Chinnery for the distribution of gold.

(1e). Irrigation and terraced cultivation. My critic says, "of course, there are none except at Bartle Bay." How about the terraced irrigation at the head of the Kikori River mentioned by Chinnery? Chinnery also mentions terraced cultivation on the Lakemaku River and irrigation on the Gibara goldfield. I inserted a mark on the Fly River because I did not wish to leave out anything that could possibly be connected with irrigation.

4. Sorcerer's charms may be due, as Mr. Murray says, to "fancy," but the word fancy must be defined. Why is quartz the chief substance used in Papua and Australia, as well as elsewhere? Surely the decanter stopper mentioned by him is a case in point. It is like quartz, and therefore is highly prized.

5. Stone images. I must refer my critic to Chinnery. Mr. Murray evidently ignores the stone images of Torres Straits and the numerous specimens found in "German" New Guinea. But he cannot get away from the fact that the stone image he mentioned was actually used for magic. To argue about numbers is surely beside the point, especially when there happen to be plenty of such images in use in the region.

6. Warfare. My critic says: "I have 'never come across a single instance that 'I can remem' r' of fighting between the two sides a dual community. See p. 293 of 'The Children of the Sun' for the Kiota a Mekeo. I shall deal with this topic in a forthcoming work. There is abundant evidence that fighting in early times in Oceania was largely between the two sides of dual communities. My phrase that is quoted by Mr. Murray needs alteration. It should read, in my present opinion,

"much of the warfare apparently consists of struggles between the communities on either side of the dual organisation." I hope before long to deal in detail with the question of warfare.

Yours faithfully,

W. J. PERRY.

Archæology.

Fawcett.

The Willendorf Venus.

To the Editor of MAN.

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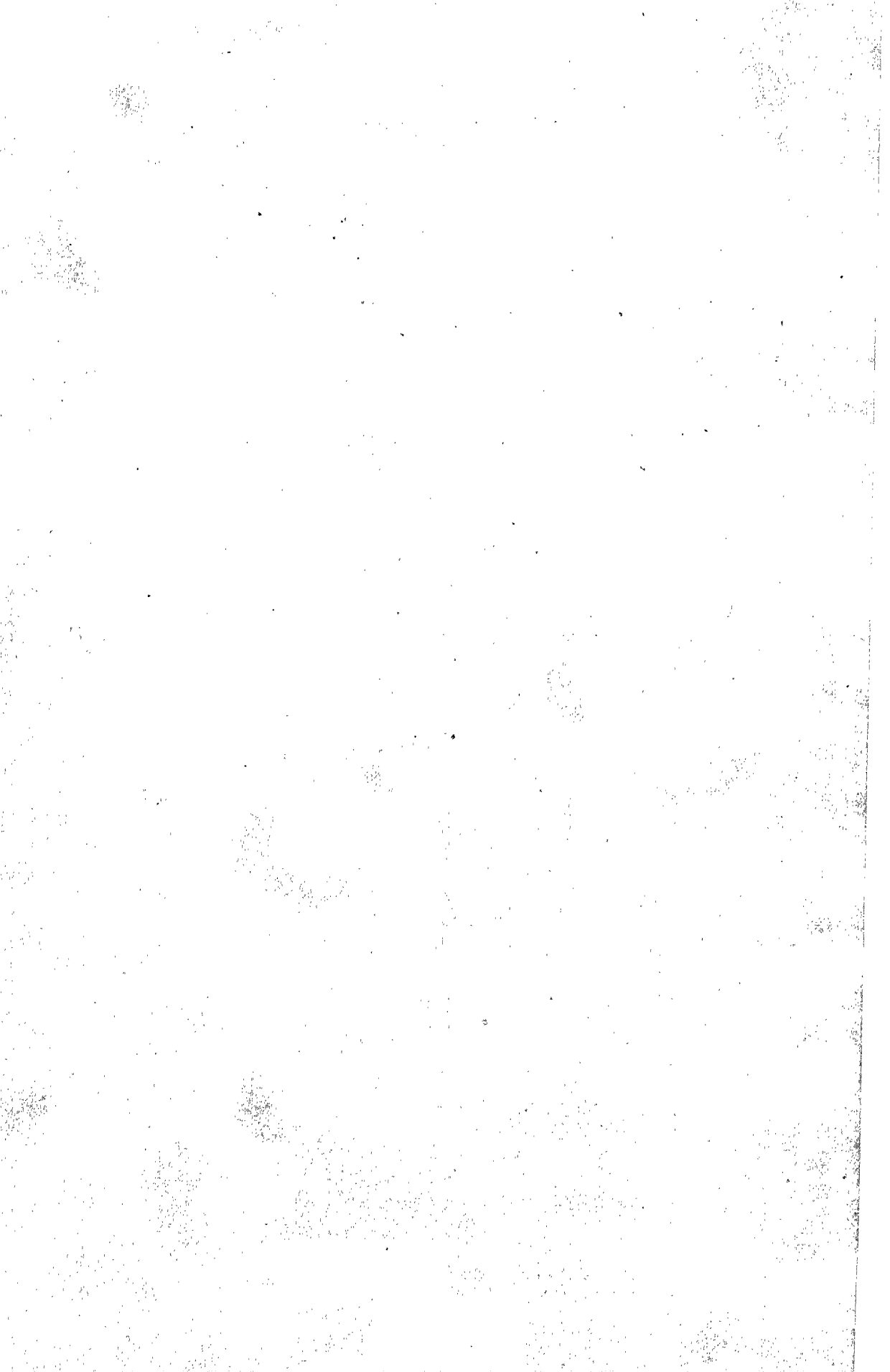
SIR,—It is well known that a stone figure of a woman, dumpy, breasts, abdomen, sexual parts vividly accentuated, was found at Willendorf in the Wachau, Austria, in 1908. Another figure of a woman, different in type of beauty, was found at the same spot in September last. Some artefacts were also found there, and Dr. Bayer has no hesitation in assigning the finds to the Aurignacian period. I visited the spot a few days ago. It lies about 100 metres from the left bank of the Danube (about 60 miles above Vienna), about 20 metres above the level of the river (now). The site is an open one between the hills and the river, on a little slope some distance from the flanking hills or hard ground, for the matrix is composed of sand and mud, clearly alluvial; in fact, the old bed of the river. Of course, it was a case of *noli me tangere*. A visitor to the site has, indeed, no need to leave the train, as he is able to see all that he can or may see within a few yards of the carriage window (on the right) as the train from Vienna slows into the little halt of Willendorf. Dr. Bayer's watchful eye over the spot noticed my footprints on the following day, so easily impressive is the ground of his precious finds. Next time he digs I hope to be there, as he has most courteously invited me to be there.

The second "Venus" is a much larger specimen than the first, being some 9 or 10 inches in length. A much slenderer figure. The left arm, of which there is only (but a clear) indication, was, obviously outstretched, lying against the body, in line with it; the right arm was flexed against the body under the breasts. The head is unfortunately lacking, and the whole figure bears signs of wear. In due time the world will, no doubt, be enlightened fully about this valuable find by Dr. Bayer, the learned representative of the Historische Museum, Vienna.

FREDERICK FAWCETT.

Vienna, 22nd October, 1926.





A book that is not a book

GOVERNMENT ANTHROLOGICAL SURVEY
GOVT. OF INDIA
Department of Anthropology
NEW DELHI

THE ANTHROLOGICAL SURVEY
OF INDIA